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THE SURGICAL TREATMENT OF LOCAL AND GENERAL PERITONITIS,

WITH REPORT OF TWO CASES OF GENERAL PERITONITIS DUE TO PERFORATION SUCCESSFULLY TREATED BY FREE INCISIONS, IRRIGATION, AND DRAINAGE.*

By W. GILL WYLIE, M.D.,

NEW YORK.

My excuse for bringing this subject before you to-night is its importance, and the firm conviction that there are hundreds of men and women dying from peritonitis who might be easily saved if the medical profession would recognize the fact that a majority of such cases could be saved by prompt surgical interference.

The number of cases occurring in every town of any size is incredible to any one who is not specially interested in studying the subject.

Here in New York, where one would suppose well-to-do patients would not be denied in many cases the only chance of being helped by laparotomy or surgical treatment, not one in ten has the question of operation considered, unless all other means fail, and then, with rare exceptions in cases of general peritonitis, it is too late.

For the past seven years I have worked faithfully with my gynæcological colleagues to try and convince them that, practically speaking, there is no such thing as chronic pelvic cellulitis; that the so-called cases of cellu-

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* Read before The New York Academy of Medicine, January 2d, 1890.

litis—especially those called chronic, where "attacks of cellulitis" are constantly "being lighted up"—are nothing more nor less than cases of local peritonitis due to salpingitis and ovaritis, *and when there is positive evidence of the formation of pus, or the patient is bedridden from frequent attacks, that the only rational treatment is by laparotomy.* That pelvic abscesses, nine times out of ten, are merely the result or the sequel of neglected cases of local peritonitis, and although when actively septic and readily reached by the vagina *behind the uterus* they should be opened and drained; but the only radical and certain cure is, as a rule, by laparotomy. And that when laparotomy is resorted to, the pus should be first carefully drawn off, and the sac rendered aseptic by washing it out with antiseptics, and not sewn into the abdominal wound, but enucleated and removed with the diseased tube and ovary nearly always found in it. Even during the acute stage of either local or general peritonitis laparotomy should be done, if life is threatened either by sepsis or intestinal obstruction.

Success has overcome almost all opposition among gynæcologists, and as a rule the above is the accepted practice. When I began I was accused of mutilating women, and even to-day some of my old teachers, while admitting my success to those who ask about me, still claim that I am too fond of the knife.

Although I do not now practise general surgery, I am interested in all abdominal surgery, and I am convinced that the general surgeons will soon adopt about the same rules of procedure in treating local and general peritonitis occurring in the abdomen that gynæcologists have established as the best plan of treating pelvic peritonitis.

My experience in peritonitis from typhlitis or perforation is limited, but my large experience in peritonitis the result of salpingitis warrants me in at least stating my views.

The general surgeons have been lately going through the discussion as to whether the abscesses formed about

the vermiform appendix and cæcum are intra- or extra-peritoneal. It is exactly similar to what gynæcologists have years ago been through in regard to pelvic abscesses. I believe that the intraperitoneal theory is the right one in both cases.

As a rule, at least, it would seem that typhlitis is due to ulceration through the appendix or intestine, causing in most cases a local peritonitis around the centre of ulceration; as pus is formed it is shut off and encysted by lymph from the free peritoneal cavity, and gradually an abscess of greater or less size is formed, unless the opening in the gut is so large, or the septic matter escaping is so virulent, that general peritonitis at once is set up.

Once the abscess is encysted, the tendency of the peritoneum is to protect itself by forcing out the pus into the connective tissue, or through the wall of the intestine, either through the original point of ulceration or a new one. This is precisely the same with pelvic abscesses around the Fallopian tubes, and in many cases nature effects a cure and helps on the conservative argument; but he must be a small man who sits by and takes the credit of curing the case by letting the patient run three out of four chances of dying, instead of operating and giving at least nine out of ten chances to get well by establishing a free and safe outlet for the pus. When the peritonitis is local the symptoms are, first, a severe colicky pain over the abdomen, in the first few hours usually most marked about the umbilicus; but soon the pain becomes most marked directly over the region of the appendix, and after the first twenty-four hours distinct tenderness will be localized in the same region. There may be vomiting, but, as a rule, the symptoms of intestinal obstruction are not present, or are only temporary in character. There are usually pretty well marked chills or chilly sensations, with some rise of temperature, usually not over 101° or 102° F.

For several days, usually four or five, the symptoms will vary, the pulse may be at times not above normal,

and the temperature may even sink to normal and the bowels move; yet the local pain, or at least pain on pressure over the vermiform process, remains, and may be the only indication of the presence of a pint of stinking septic pus which is liable at any moment to burst into the free peritoneal cavity and cause death in twenty-four hours. This condition was perfectly illustrated by my second case. Usually on the fifth, sixth, or seventh day the abscess will find an outlet or burst and cause general peritonitis.

As a rule, if the case starts as one of general peritonitis, it will not end fatally in twenty-four or forty-eight hours, as is the rule when a large septic abscess bursts after five or six days of local peritonitis, but the colicky pains will be more intense and persistent than in local peritonitis, and the vomiting and constipation or intestinal obstruction will be persistent or readily provoked by attempts to feed, etc.

The patient may live four or five days, or even longer, if kept quiet and under the influence of opium, but, unless operated upon before marked shock is present, will die with absolute certainty.

The following are short histories of the five cases of typhlitis and perforation that I have had to deal with. They fairly illustrate the most important points in local and general peritonitis starting from that locality.

I have operated upon more than two hundred cases, either during or while the patients were suffering from the results of local peritonitis starting from the Fallopian tubes or ovaries. In quite a number of these there was suppurative general peritonitis, and operation was performed when pulse was 110 to 120, and temperature 103° to 104½° F.; but such cases with gynæcologists are no longer rare, and the necessary steps of operating are now very generally accepted. But it is not so with local and general peritonitis starting from the vermiform appendix or cæcum, which are usually treated by physicians and operated upon by general surgeons. In several cases of

salpingitis I have found the appendix adherent to the tube or ovary, and thrice I tied it off for hæmorrhage from it when the adhesions were separated, and all of these recovered.

CASE I. *Intestinal Perforation; Local Peritonitis; Perityphlitic Abscess.*—Negro man, aged twenty-two, was admitted to Bellevue Hospital, August 24th, 1885. He was sent to the medical side as a case of peritonitis. In the absence of my colleague I was asked to see the man. He said that for about a week he had had great pain over the abdomen, with considerable fever, sweating, and chilly feeling. He had a temperature of 102½° F., with a quick pulse and rapid respiration. His abdomen was distended, hard, and tense, and very sensitive to the touch, especially over the right iliac region, where he said was now the point of greatest pain. He had been constipated and vomited some, but his bowels moved with an enema. There were some, but not positive, signs of the presence of a tumor. I decided that there were indications of pus, and he should be operated on at once. Dr. Janeway and Dr. Stimson saw the case, but differed with me about the indications for an operation. Later in the day the late Dr. McBride saw the man, and he agreed with me that there was pus. I had the patient etherized, and could then readily make out the presence of a tumor the size of a cocoanut near the crest of the right iliac bone, within half an inch of the bone. I made an incision about three inches long. When the peritoneum was reached, I dissected down near the bone, so as to get somewhat under the side of the tumor, and to be sure to open it where it was firmly adherent to the parietal peritoneum, for I wanted to prevent the escape of the pus into the general cavity of the peritoneum. I found the muscle grayish in color, the change from the normal red color being due to slight infiltration from proximity to the inflamed tissues. I have since found this grayish color of the muscle to be a good guide in seeking a point where the sac of the tumor is adherent. With a pair of sharp-pointed forceps I carefully opened the sac of the abscess, and at once a strong spurt of fetid pus gushed out; at least a pint of dirty-colored pus with strong fecal odor escaped. The cavity was well washed with a solution 1 to 3,000 hydrarg. bichlor., and a double drainage tube inserted. The patient's pulse fell

to normal, and he made an uninterrupted recovery, and left the hospital in three weeks.

CASE II. *Intestinal Perforation; Local Peritonitis; Perityphlitic Abscess.*—Male, aged thirty-seven; a strong, vigorous man. While sitting in a saloon had a violent cramping pain of the abdomen; he took some hot spiced drinks and was carried home. He suffered intense pain and sent for me the next day. His abdomen was distended and sensitive, and he had vomited some; the pain was most marked over the umbilicus; his temperature was 101° F., and pulse 105; respiration, 25. I gave him a hypodermic of morphine, and ordered a laxative for the next morning. The next day the pain was less, but more decidedly sensitive over the right side; his temperature was only 100° F., and he felt generally better after his bowels moved. The third day he had a chilly sensation, and temperature was 102° F. I kept him in bed and on a milk diet. The fourth day he was better, but still had decided pain over the right iliac region, and was constipated. He was very fat, with not less than three inches of adipose tissue over the abdominal walls, and I could not make out a tumor, nor could I make out anything by rectal examination; but on account of the pain and decided sensitiveness over the region of the cæcum I determined to call a consultation, and refused to call a medical man. Dr. L. A. Stimson was selected. He could not make out any tumor, but agreed with me that the patient should take ether, and, if necessary, have an incision made. He was ordered a dose of castor oil. On the morning of the fifth day his bowels moved freely and without trouble, his temperature and pulse were absolutely normal, but, as the local pain was still very marked, he was given ether. When fully relaxed we could, through all the fat, make out indistinctly a tumor near the ilium. A free incision about three and a half inches long was made, and, when the peritoneum was reached, it was plain that we were in contact with a large, fluctuating mass. Selecting a point as far back as could be readily reached, I opened the peritoneum with sharp-pointed forceps, and out gushed a strong stream of stinking pus with a very decided fecal odor. The opening was enlarged, and the abscess well washed out with 1 to 3,000 hydrarg. bichloride. There was no escape of fæces or gas, and when my finger was in I could feel the walls of the abscess, but made out nothing of importance. A double drainage tube was inserted, and the upper end of

the incision closed. The patient never had any rise of temperature or pulse, and in three weeks was sitting up. During the second week a nucleus of hard fecal matter, with ground-up seeds like those of strawberries, came out of the wound, and soon it closed up from the bottom. There has never been any return of the trouble, but a decided hernia exists, and a truss is necessary to prevent its increase in size.

CASE III. *A Case of General Peritonitis, due to Bursting of a Typhlitic Abscess* (October 26th, 1888).—This case was fully reported by Dr. A. M. Jacobus in the *Medical Record*, February 2d, 1889. Man, aged fifty, in good physical health, was taken with severe colicky pains on Saturday. On Monday the pains became very severe; on Tuesday he sent for Dr. Jacobus. The doctor found him lying with his knees drawn up, the abdomen tense and very sensitive to the touch, especially over the cæcum; he was constipated. He was ordered to bed and one-eighth grain of morphine and three-eighths of a grain of antipyrin given every three hours as indicated. Saline laxative was given the next morning. Wednesday he was better, had a full stool, but was still in pain, especially on the right side; temperature 100⅓° F. Thursday afternoon he grew worse, and the doctor detected some signs of a tumor over the region of the cæcum, which confirmed his diagnosis of perityphlitis. Friday, the fifth day, at 6:30 A.M., he was taken with violent cramping pains and rapidly developed all the symptoms of general peritonitis. At 9 A.M. he had a violent chill, and temperature went to 102° F., and pulse to 110. At 11:30 I saw him, and decided to operate at once. The abdomen was exceedingly tense and very hard, but no signs of tumor could be detected under ether. I opened the abdomen in the median line just below the umbilicus, and, as soon as the peritoneum was opened, out gushed a quantity of purulent serum and flakes of lymph. Introducing my fingers, I made out a collapsed abscess sac over the appendix vermiformis. I then made another opening through the abdominal wound directly over this point. I inserted a large-sized glass drainage tube, attached to the tube of a large fountain syringe, and washed out the whole abdominal cavity. Introduced my hand and separated the adherent coils of intestines, and again washed out the peritoneum. I found puddles of pus scattered among the coils of intestines which even the second washing out did not

reach, and again went over the abdominal cavity with my hand, and again washed out till the water came away perfectly clear. The pus sac was freely opened, and the bean which had ulcerated was found. As there was no escape of fecal matter or gas, the adhesions about the appendix were not disturbed. A glass drainage tube was inserted through both the median and lateral incisions, the latter going directly into the remains of the abscess sac. The wounds were closed with silk and dressed in the usual manner. In less than two hours the temperature fell to normal and the pulse to 90. The case was treated in the usual way, nothing by the mouth for thirty-six or forty-eight hours, warm rectal enemas for thirst. The drainage tubes were sucked dry every few hours. The central one was removed on the third day, and the lateral on the eighth day. The sixth day the sutures were removed, and, while angry with his nurse, the patient raised on his elbow to drink a glass of water and burst the median incision, the intestines escaping from under the dressings. They were carefully washed, returned, and in an hour I closed the wound with silk sutures without ether. The temperature rose slightly, but a laxative brought it down, and he made an uninterrupted recovery, and has been well since then.

CASE IV. *Intestinal Perforation of Cæcum; General Septic Peritonitis.*—J. McG.——, aged twenty-eight; good family history, strong and healthy, never had venereal disease, good habits; mechanic. February 5th, 1889, he had some heavy lifting to do, but quit work at 6 o'clock feeling all right, ate a simple supper. About 9 P.M. he had a very severe cramping pain, most marked over the right side of his abdomen. By 2 o'clock the pain was agonizing, and Dr. Ernest Walker, living next door, was sent for, who gave him hypodermics of morphine, one-sixth grain, and one grain calomel by the mouth, until the pain was in a great measure relieved.

The next morning the whole abdomen was extremely sensitive to touch, and the abdominal walls as hard as boards, with some distention. The pain was very severe and decidedly more marked over the region of the umbilicus. During the day vomiting set in. More morphine was given and attempts made to move the bowels by enemas, but they were retained. Hot applications were made over the abdomen. Thursday the pain was still severe, and most sensitive over right side, and temperature up to

101° F., pulse 110. Four seidlitz powders an hour apart and an ox-gall enema were given, but all of no avail. Some spinach-green vomiting. I saw the patient Thursday; his general condition was good, but all symptoms of general peritonitis were well marked. I had him sent to Bellevue Hospital. Before operation enemas were given, but were retained; rectal tube let off gas and gave some relief. Temperature was only 100.6° F.; pulse, 106; but respirations were 30. Ether was given. As no signs of a tumor could be made out, I opened the abdomen in the median line just below the umbilicus. When the peritoneum was opened abundant purulent serum poured out as soon as the adhesions of lymph were broken up with the finger. In passing the index finger over toward the cæcum the pus became more abundant and had a decidedly fecal odor. I could not make out any signs of a local abscess, or sac of one, but decided that the region of the cæcum was the starting-point of the peritonitis. I then made an incision through the abdominal wall just over the cæcum. Through the median incision I introduced a large-sized glass drainage tube, which was attached to a large fountain syringe containing two gallons of hot water at 112° F. I handled the tube so as to wash out all the excess of pus about the cæcum. I then broke up all adhesions of the intestines in every part of the abdomen and pelvis except about the cæcum, and washed with six or eight gallons of hot water till it came out perfectly clean. By opening the lateral incision the point of perforation in the cæcum, sealed over with lymph, could be plainly seen. It was circular in shape and about two-fifths of an inch in diameter. The appendix pointed upward, was somewhat adherent, but apparently nearly normal. A glass drainage tube was put in both openings and the incision closed around them in the usual manner with silk sutures, and the wound dressed with iodoform gauze, absorbent cotton, etc.

The operation lasted about one hour, there were no signs of shock at any time, and the patient's pulse was as good, if not better, after than before the operation.

He made an uninterrupted recovery, his temperature was never above 101° F., with pulse 104, and after the third day it was practically normal; tympanites and pain had entirely left him. The glass drainage tube in the median line was removed the third day, but the other tube drained rather freely and had some fecal odor for several

days. The edges of the wounds, especially the lateral one, sloughed and gaped on fifth and sixth days when the stitches were removed. The discharge was irritating, and the peritoneum seemed, when assisted by drainage, to take better care of itself than the connective tissue and muscles. I have noticed this in all of my four cases of intestinal perforation. The sloughing incisions were marked in all of them, while in peritonitis from salpingitis it is exceedingly rare. Thirty hours after operation a simple enema was given and bowels moved freely without trouble. After this one small dose of morphine was given, and it was the only one after the operation. After bowels moved, kumyss and pancreated milk were given. On third day his bowels moved very freely and the amount of food was increased. On account of the lateral incision closing as an open wound he did not sit up till the end of the third week, but went out of the hospital, perfectly well, at the end of five weeks.

CASE V. (September 28th, 1889).—I went to the northern part of New York State to see a woman, forty-eight years of age, who had in the past ten days developed a tumor, which was very painful, on the right side just above the crest of the ilium. The history was the usual one of a slow developing abscess about the vermiform appendix. Before I saw her several ounces of pus had been drawn off with an aspirator. I could plainly make out a tumor the size of a large cocoanut. The tissues near the ilium were somewhat indurated. She was etherized, and I made an incision near the crest of the ilium, extending backward; when I reached the muscles they were grayish and infiltrated. It was evident that the sac of the abscess was firmly adherent to the lateral wall of the abdomen, and that I could safely open the abscess cavity without entering the general peritoneal cavity; this I did, and out gushed about a quart of fetid pus. I inserted my finger, enlarged the opening, and put in a double rubber drainage tube. The tubes drained freely for several days and were then shortened, and in the fourth week the wound was healed, and patient got well.

Conclusion.—When there are symptoms of local peritonitis, intense pain and tenderness, followed by tympanites and vomiting, with chilly sensations and rise of temperature, search should be made for the cause. As a rule, the

pain will soon become localized over the region of the Fallopian tubes, the cæcum or gall bladder, or some old ventral or inguinal hernia. If signs of a tumor or exudation can be definitely made out, and the general symptoms indicate the formation of pus, then the patient should be etherized and the pus reached by incision, the pus evacuated, the cavity washed out and drained. If it is in or involves the tubes and ovaries, the abdomen should be opened; and if the tube or tubes are occluded and pus found, they, with the ovary or ovaries, should be removed. If the abscess is around the appendix vermiformis or cæcum, an incision should be made near the crest of the ilium and the peritoneum dissected up till a place is reached where the peritoneum is adherent to the tumor, which should then be carefully opened, the pus evacuated, the sac washed out, and drainage tube introduced without opening into the free cavity of the peritoneum. If it is about the gall bladder, if the signs of pus can be made out, an incision over the sac should be made and the pus evacuated.

If the general symptoms are severe and no localized centre of pus made out, then an incision should be made in the median line and the peritoneal cavity explored with the index finger. If then a pus sac is found, if it be so situated that it can be reached by another lateral incision and the pus evacuated without allowing the pus to escape into the free peritoneal cavity, it should be made and the median incision closed. If it cannot be reached by a lateral incision where the wall of the sac is adherent to the abdominal wall, then the pus should be drawn off from the sac by an aspirator or trocar, and the cavity washed out clean with an antiseptic solution before it is freely opened, and a drainage tube inserted.

If signs of general peritonitis show themselves, that is, by vomiting, obstinate constipation, tympanites, etc., then a free incision should be at once made in the median line and the starting point of the peritonitis found, if possible. If it is over the cæcum an incision should be made over it,

and the pus washed out by means of hot water of a temperature of 110° to 115° F., run from a large fountain syringe with a large-sized glass drainage tube attached to the rubber. After the free pus about the cæcum is well washed out, several fingers or the whole hand should be put in the abdominal cavity, and the intestinal adhesions broken up, and all puddles of pus completely washed out. Then a glass drainage tube should be introduced into each opening and the wounds closed around them, etc.

In pelvic peritonitis, as a rule, the operation is not necessary to save life; but it may be, and it is better to operate during the first attack, if there are symptoms plainly indicating the formation of pus, for the adhesions are much more easily broken up, and more complete removal of the diseased organ can be done, than after contraction and dense adhesions have formed, as they do after repeated attacks of inflammation. Besides, dangerous pelvic abscesses are avoided and the bad influence of chronic invalidism are prevented. Of course, I refer to severe cases of local peritonitis, where there are symptoms either subjective or objective, indicating beyond reasonable doubt the presence of pus.

In typhlitis the operation should be done before the fourth day—the earlier the better, so as to prevent the chance of rupture and septic general peritonitis, which is, as a rule, attended by so much shock that no operation can do any good. As a rule, general peritonitis from rupture of a septic abscess is likely to be attended with more shock and rapid failure, and death, than peritonitis from direct perforation.

In all cases of general peritonitis an exploratory incision should be made as early as possible, after trying to lessen the tympanites. If an exploratory operation does no good, it is not likely to add much to the danger.

There may be cases of idiopathic peritonitis, but I have never seen one proved by anything to be relied upon. Certainly in septic peritonitis, where shock is not too great, free opening, washing out, and drainage will cure

some cases. It helps, if it does not cure, tubercular peritonitis, and exploratory incision has proved to be, in the hands of experts, almost entirely free from danger, and it must become the practice in almost all cases of general and severe cases of local peritonitis where there are marked symptoms of the formation of pus, an extension into a septic peritonitis, or intestinal obstruction. Many cases of local peritonitis due to salpingitis may never necessitate a laparotomy, for they often become encysted and give comparatively little trouble. Not infrequently the symptoms are due to a severe metritis, where the peritoneum covering the enlarged and inflamed uterus becomes so sensitive as to give in a marked degree almost all of the symptoms of a peritonitis due to salpingitis, and repeated attacks of this kind are often mistaken by good physicians for genuine cases of salpingitis. But I am referring here to cases where a distinct tumor can be felt in the broad ligaments, accompanied by other symptoms indicating the formation of pus, which, when not operated on, rarely fails to cause a general peritonitis, and kills or makes the patient a confirmed invalid.

In typhlitis the diagnosis is, as a rule, comparatively easily made, and I believe it will soon become the general practice to operate early in all well-marked cases. What I wish to especially advocate to-night is early operation in cases of general peritonitis, both those starting from a local peritonitis and those due to the escape of septic matter into the peritoneum, and to make it plain that to succeed in such cases it will not do to merely open the belly, allow pus to escape, put in a drainage tube or gauze, and leave intestinal adhesions causing obstruction to remain to kill even more certainly than septic poison, or fail to empty and wash out all puddles of septic fluid encysted among the coils of intestines; but we must make free incisions large enough to introduce the hand, to break up all adherent intestines, and freely wash the whole cavity of the peritoneum, and put in two or more drainage tubes. Without question, many cases where the patients

are debilitated weeks before the starting of the peritonitis, and in those cases where the septic poison is too virulent to be successfully washed out, our patient will die in spite of an early operation. But are the chances of this class of cases getting well, lessened by the operation? I think not. Death may be hastened, but that is all. But the majority—the large majority—of cases of general peritonitis taken early are not in this desperate condition, and yet, if not operated on, at least four out of five die. Probably many of you may think that the two cases of general peritonitis that I have just reported were mild cases, and some will think that they would have recovered had no operation been performed; but recollect that one was operated upon only a few hours after the septic abscess had ruptured, and the other within seventy-two hours after the not so septic contents of his intestines had escaped into the peritoneal cavity. I did not wait till they were in a desperate condition, but they had every symptom of general peritonitis, and had you been present and seen in the cavity of the peritoneum, as I did, the amount of pus and of lymph breaking down into pus, and the condition of the intestines, you would not doubt my statement. The second case had complete intestinal obstruction and the characteristic spinach-green vomit.

THE MINERAL WATERS OF AIX-LES-BAINS AND MARLIOZ.

PRACTICAL CONSIDERATONS ON THEIR ACTION AND APPLICATION.*

By L. BLANC, M.D.,

AIX-LES-BAINS, FRANCE.

THE town of Aix-les-Bains is situated in one of the most picturesque alpine valleys, on the border of Lake Bourget, and surrounded by mountains which shelter it from north and east winds. The climate is mild, the average temperature being 55° F., and the vegetation nearly the same as that of the south of France.

The medical history of the town is of ancient origin, for, besides the temple of Diana, the arch of Campanus, the Roman baths and monuments which still exist, traces of numerous baths, swimming baths, and Latin inscriptions have been found in digging for the foundation of the new baths; all of which tends to show that under the Roman government the thermal establishment was highly important.

At present the bathing establishment is one of the most important in Europe. It is situated in the eastern part of the town, at an altitude of seven hundred and eighty-six feet above the level of the sea, and on a hillside, and is divided into three successive stories or terraces, which have been utilized for the application of the waters.

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* Address delivered at a meeting of the Academy of Medicine, in New York, on Thursday, January 2d, 1890.

There are fifty-one douche chambers where the douche is given by one or two persons, called *doucheurs* or *doucheuses;* two needle douches; three local douches; four vapor local douches, called *douches Bertholet;* two vapor boxes for the whole body; sixteen vapor baths called *bouillon*, ten of them attached to douche divisions and supplied with natural vapor coming from the springs; forty-eight bath rooms, ten of which adjoin douche rooms; six swimming baths, two of them especially for children, and one reserved for skin complaints. There are two inhaling rooms, one of which is provided with contrivances, called *tambours*, and india-rubber tubes, to convey vapor to the lungs, eyes, ears; process termed *humage;* four foot baths, and three drinking fountains.

Each douche is provided with numerous apparatus, of which the most remarkable is the mixing box, *boîte de mélange*, arranged so as to distribute the water during the douche at an unvarying temperature and pressure.

The douches bear various names, and vary in nature. Some are supplied with hot water only, some with hot and cold water, some with the two hot springs and cold water combined. The ventilation varies according to the douche. The pressure of the water varies from eighteen feet to forty-two feet.

In fact, the physician has at his command all the advantages of varied apparatus, temperature, ventilation, and pressure, which enable him to modify at will physiological results and to guide his patients through the course of treatment, thus avoiding mishaps which might occur in any establishment not so well fitted up.

Added to so complete a system of douches with natural hot mineral water, is the massage (shampooing), which is performed with rare skill by doucheurs, who each year are specially instructed in the art, both in theory and in practice.

The massage, owing to the immense quantity of water to be had at will, is done with the aid of a continuous flow of water which gives to the *Aix douche* its special

feature. But, to understand well, it is necessary to give an exact description of the douche.

This is how it is administered: The bather being seated on a chair, the water falls on all parts of his body successively, with more or less force of volume, according to the prescription of the physician. Two doucheurs during this time rub the skin, shampoo (or *massent*) all the muscles, and make him move his different joints in every direction. This operation, which lasts from five to fifteen minutes, being over, the bather is wrapped in a flannel gown, sheet, and blanket. Once in this *maillot*, as it is called, he is placed in a sedan chair, conveyed to his house, where a warm bed awaits him, and there is confided to the *sécheur* of the house, who wipes him down, leaves him more or less time in the *maillot*, and gives him some mineral water or other beverage to drink, according to the directions of his doctor.

By this manner of administering the douche, it is seen that the attentions with which the bathers are surrounded at the establishment are continued in their residences.

At other times, after the douche the bather, well dried and wiped, takes a walk and brings on the reaction by exercise.

This douche may be infinitely varied with regard to the massage, the temperature, the volume of the waters, or the force and direction of the fall. The massage may be exercised partially, or on the back of the body in general, the head excepted; it may be more or less energetic; it may be accompanied by friction with the hands or by brushes.

As regards the temperature, the douches may be taken from 61° to 116° F. The bather may pass insensibly from one degree to the other, as he may pass abruptly from an elevated to a low temperature, and *vice versa* (*douche écossaise*).

He may also be subjected to different temperatures at once. He may receive, for instance, hot water on the feet, tepid on the shoulders, and cold on the head. The

bather may receive water on all the body, in torrents or in feeble quantities. This water may fall with force in jets, in sprinkling, or in streams; it may be conveyed by pipes furnished with tubes having larger or smaller openings, or terminated by a sprinkling-rose with holes more or less fine.

It may be directed horizontally to the hip, or douches may come from below upward to the perineum (*bec de corbin*), or vertically, that is to say, in any direction.

Finally, the pressure of the water may be varied infinitely, as I have shown above.

From this variety in the administration of the douches arises the action which I call mechanical, and by which we may produce physiological effects, sometimes stimulating, sometimes sedative, sometimes disturbing, at others revulsive—in a word, which can be modified infinitely.

Two thermal sources supply these douches, vapor baths, etc., one called sulphur, the other alum (though containing no alum), to the great amount of nearly one million of gallons in twenty-four hours, to which are added half a million of gallons of plain cold water.

Both springs are sulphurous, and contain sulphuretted hydrogen in equal quantities, besides several other elements, such as carbonate of iron, carbonate of magnesia, lithia, etc. There is an organic matter called *glairine*, which makes the water very soft and is a great help for good massage. The temperature varies from 112° to 116° F. (they mark 4° by the sulphydrometer Dupasquier).

There are one hundred and ninety-two men and women employed in this extensive and important establishment, eighty-four of them being masseurs or masseuses.

A variety of physiological effects is produced at the same time, through so thorough and so varied a treatment. Besides the special action of the water itself, well known as most beneficial in cases of rheumatism and gout long before the introduction of the present forms of apparatus, results of a very different nature are obtained by

the combined action of the douche with massage, such as an exciting, a sedative, or a revulsive effect; but the principal one is the regulating of the organic functions, such as the circulation of the blood, and the appetite. By this means we remove the morbid conditions which are the chief causes of the two principal diseases for the cure, or benefit, of which Aix is famed, viz., rheumatism and gout. In a word, more or less of uric acid is eliminated as long as the treatment lasts, as has been proved by the works of Dr. Monard and Dr. Forestier.

The mechanical action of the massage and physiological movements in water or in vapor accounts for the successful results obtained in diseases of the joints, whether constitutional—hydrarthrosis, arthritis, etc.—or arising from an accident or after an operation—contusion, fracture, wounds, dislocation, etc.

In these affections medical intervention is indispensable to direct the doucheurs in the operations; the *massages*, the movements especially, must be watched with greatest attention. It is for these reasons that the physicians at Aix are accustomed to stay in the bath establishment from 6 to 11 A.M. every day.

Though not mineral to a high degree, the waters of Aix, with help of those of Châlles, and particularly of Marlioz, which forms part of Aix, are made use of in catarrhal complaints, in those of the lungs, and also in skin diseases which require strong sulphurous thermal treatment.

The hydro-therapeutic treatment of Aix (douches and swimming baths, together with drinking the waters of Marlioz, which are highly charged with iodine and sulphur, as well as breathing the vapor arising from these waters) forms a potent cure for children suffering from bronchial swellings, or from repeated bronchitis, which lays so ready a basis for consumption.

It also triumphs over syphilis or disorders arising from mercurial poisoning. In a work published in 1887, I have laid down in a precise manner the indications for

the use of sulphur in the treatment of syphilis. The conclusions at which I arrived are the following:

1. Sulphur and sulphurous combinations do not themselves cure syphilis, but they have an incontestable influence on the complications of this disorder.

2. By their stimulating action they draw to the skin the syphilitic manifestations, and consequently facilitate the diagnosis and direct the physician in his therapeutics. They act on the organism, during a course of mercurial treatment, in many ways, by carrying the medicaments to the parts where the evil exists—that is to say, by producing the elimination of the mercury through the skin, by opposing the mercurial accumulation, and consequently the accidents which spring from it, diarrhœa, stomatitis, cachexia, and curing them when they have been produced.

3. By the cutaneous irritation which they produce, the sulphurous combinations have many effects. They accelerate the circulation of the blood and lymph, especially when scrofula is at work; thus they dissipate the ganglionic obstruction, and in fact they promote the reconstitution of the blood.

4. If such patients have rheumatic or gouty symptoms, syphilis and the other constitutional disease can be cured together.

It would take too much time to repeat here the experiments I made; at this time a summary will suffice.

If we pour a solution of hyposulphite or sulphite of sodium on a precipitate of albuminate of mercury, this precipitate is invariably dissolved.

The same phenomena take place in the human body.

In presence of alkaline salts and oxygen, the sulphur absorbed is transformed in the blood into hyposulphite and sulphite, which, when brought into contact with the albuminate of mercury fixed in the body, dissolve it and facilitate its elimination. I have taken, in fact, two classes of patients, whose urine I have examined every two or three days for a period of twenty days:

1. Patients undergoing at the same time the thermal and mercurial treatment. With these patients the results have always been positive; that is to say, mercury has always been found in the urine in an appreciable quantity.

2. Patients who have not taken mercury for one, two, or three months. I have examined the urine four or five days before commencing the sulphur treatment, and I have found no traces of mercury in the urine. I have afterward prescribed the sulphur treatment, *intra et extra*, and, after a lapse of time varying from four to eight days, I have found, with two-fifths of the patients treated, mercury in the urine; which proved in a manner absolutely certain that mercury had accumulated in the body, and that this mercury was eliminated under the influence of the thermal sulphur treatment.

Reviewing a series of 1,000 patients during five years, I find:

Rheumatism	453
Gout	94
Diseases of the joints.	102
Sciatica	61
Pharyngitis, laryngitis, and bronchitis	59
Syphilis	48
Metritis	27
Neuralgia	12
Gastralgia, dyspepsia	13
Cutaneous complaints	23
Various nervous complaints, palsy	24
Old bruises and broken limbs	36
Myxœdema	4
Various—contraction of the tendons, paralysis, ozæna, phlegmasia alba dolens, glycosuria, lead poisoning	33
	1,000

As it is easy to see, the more important diseases treated at Aix are *rheumatism* and diseases of the joints in all their forms, from simple rheumatoid pains to the most severe rheumatoid arthritis, as well as all the complications which form part of the maladies—neuralgia, sciatica, dyspepsia, gastralgia, pharyngeal and bronchial affections, and even cardiac complications.

In speaking of *gout*, I refer principally to chronic gout of the joints, but also to gout which manifests itself in cutaneous eruptions, and to those cases, called suppressed gout, in which the circulation of the blood is low and the manifestations of the gout itself very inactive.

In speaking of the treatment of rheumatoid arthritis, Dr. Garrod says that waters which have been found very beneficial in rheumatoid arthritis are those of Aix-les-Bains, at which place the douche is a very important part of the treatment.

In another book Sir Alfred Garrod says: "1. Reviewing the results in the very numerous cases sent by me during the last twenty or more years, I am strongly of opinion that more benefit has been derived from the treatment at Aix than would have been received at any other spa I am yet acquainted with. 2. That in many diseases of the stomach, liver, bladder, etc., the value of several other courses would often exceed that of Aix-les-Bains. 3. That in rheumatoid arthritis—the rheumatic gout of old writers—the value of the Aix-les-Bains course far exceeds, according to my experience, that of any other known spa. 4. That in many other articular affections, such as gout and rheumatism, the Aix course is very valuable, but not to the same extent as in rheumatoid arthritis; and that an equal benefit may often be received from some other mineral-water treatment."

As for the other diseases I have mentioned above, it is easy to understand the successful effect obtained by combining the mineral waters of Aix-les-Bains and Marlioz; the temperature of the waters and the various manners in which the process of douching is performed, the varied and powerful system of hydro-therapeutics made use of, produce the therapeutical and physiological results above mentioned.

The establishment is open all the year round, but the most favorable time for undergoing a course of treatment is from May until the end of October.

I must add that the town of Aix-les-Bains, apart from

the spring, is a healthy resort. The mildness of the climate, the varied excursions which can be made on the mountains, lake, etc., and the distractions found in the town, form useful agents in the cure of many ailments.

The waters of Marlioz, to which I have several times referred, are situated half a mile only from Aix, and contain sulphur, iodine, and bromine, and several alkaline salts. The bath establishment of Marlioz contains two large inhaling rooms, nine bath rooms, eight complete apparatus for atomized spray inhalation, a cold douche, four hip douches (*douche de siége*), two ascending douches, and two drinking fountains.

The alkaline waters of St. Simon, which are about equally near, can well replace the waters of Contrexeville, Vitel, and Evian in the treatment of kidney diseases.

TREATMENT OF TYPHOID FEVER.*

By W. H. THOMSON, M.D., LL.D.,

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It is a question of much interest whether we are able materially to modify by treatment the course of an acute disease commonly marked by a high percentage of mortality. Like all important questions in therapeutics, it is not easy of solution. In the case of typhoid fever we seem to have some advantages for such an inquiry, owing to its exceptionally long course enabling us to note the effect of our procedures better than we can in the shorter and often apparently uncontrollable development of some other affections, as acute pneumonia. Hence, at first sight the statistics of hospitals appear to offer a prospect of reaching some conclusions on this subject, because of the greater number to be found there than the most extensive private practice can afford, and so grouped together as to allow of estimating such elements as variations of type from year to year, the patients being also under the same conditions of treatment and attendance; and finally because of the better facilities in them for post-mortem examinations. These advantages, however, too often prove on investigation rather to apply to the study of the pathology of the disease than to its treatment. To begin with, a large proportion of the typhoid-fever cases in our hospitals do not apply for admission until after the disease has been running its course for some time, often not until their friends or attendants have been worn out by the care of nursing them, so that not uncommonly they are sent to the hospi-

* Read before The New York Academy of Medicine, January 16th, 1890.

tal in a dying state. Now, in such a slow fever as typhoid, if a patient does not survive the fifth day after admission his case can hardly be taken fairly into an estimate of the probable effect of the hospital treatment, any more than a case dying within such time from the effects of a burn or of a railway accident could be allowed to affect the value of its surgical statistics. Another element of uncertainty in thus judging any one mode of treatment is due to the comparatively brief term of trial possible for it, owing to the organization of most, at least of our New York, hospitals. In the Bellevue and in the Roosevelt hospitals, with which I have been connected for a number of years, the gentlemen on the medical staff attend in no case for more than two months consecutively. Any one, therefore, wishing to test a particular line of treatment begins his service with a series of patients who already have been treated otherwise for a greater or less length of time, while at the close of two months he leaves a number of his histories correspondingly incomplete, so that any deductions of a statistical kind from such an experience cannot be regarded as free from many of those elements of uncertainty, if not of fallacy, which beset all statistics of therapeutics. In pathological observations, on the other hand, we have little else than accurate arithmetical computation to deal with. The percentage of cases with epistaxis, diarrhœa, intestinal hæmorrhage, pneumonia or other visceral complication, can be as readily computed as any other percentage, but whether, under the conditions specified, this or that remedial measure be capable of estimation by the numerical method will always remain problematical.

My experience with this fever, however, dates from the beginning of my medical practice, when in 1860 I held the position of physician to the Quarantine Hospital of New York, where I had the fullest opportunity to study this disease in hundreds of cases of its typical European form in patients landed from crowded emigrant vessels. Since then I have had opportunities of noting its various modi-

fied developments as it occurs in our own country, both in our army during the war and in private and in hospital practice in New York; and though I have frequently kept notes, or had them taken and tabulated by hospital assistants for statistical purposes, yet I think it would be more advantageous on this occasion to consider some points of treatment from a clinical rather than a statistical aspect. I do this the more from a conviction that this disease is peculiarly unconformable, in its different development in different individuals, to mere numerical notation. One case of typhoid fever may be virtually as unlike another case as one of bronchitis differs from one of pleurisy, and therefore as little fit to be entered in the same column of figures. I prefer, therefore, to discuss the various problems of therapeutics which arise in the nature of the complications with which we most commonly have to deal in the course of this fever. In fact, this is the chief duty of the physician in the treatment of acute diseases in general. We cannot cut them short or cure them, but must rather let them run their course. In them we are much in the same position as a driver with a runaway horse, whose business then is not to try to match his strength against that of the horse and thus bring him to a sudden stop, but rather to keep him in the middle of the road, and by a timely pull on this rein and then on that to avoid this ditch or that stump until the beast blows his breath out which he assuredly will after a while. It is, of course, a great advantage if the man knows the road well beforehand, and can therefore judge in time what particular danger must be watched for. But nevertheless the difference is great between different runaways. One can be kept readily to the middle of the road. Another zigzags from side to side or seems bent on going down that steep bank ahead. And equally so is it useless to speak of the treatment of different cases of scarlet fever, of diphtheria, or of typhoid fever, as showing the same thing in their management, when they may scarcely be the same in anything else.

The first complication which I would refer to is the danger of starvation. This is a peril which is sure to arise more or less in any long-continued fever, from the direct effect of the fever process *per se* in deranging or arresting the function of secretion in general. The skin becomes dry, the urine scanty, and the bowels constipated from the first rise above the normal temperature point of the blood, at which the entire system of the secreting glands is adjusted, and nowhere is this effect more pronounced than in the case of the digestive secretions. The stomach then is not able to take solid food, not because it is heating, as it used to be termed, but because there are no digestive secretions there to deal with it. But in typhoid fever this danger is particularly intensified by the specific processes of the disease itself, for the original seat of its development is in the alimentary canal. In fact, it should be named the *febris enterica*, or abdominal fever, as it not only arises there but works its most serious textural changes in the apparatus itself of alimentation. It is by no means the glandular tract of the intestines only which suffers, for recent researches show that in no disease, except in gastric cancer, is the peptonic power of the stomach so decreased as in this fever. Dr. Samuel Fenwick, of the London Hospital (*Lancet*, June 11th, 1887), reports four cases of marked atrophy of the stomach in typhoid fever, in which he carefully scraped off the whole of the gastric mucous membrane and proved that the average weight was only 580 grains, while the average weight of the same membrane in seventeen patients who had died of other diseases was 1,035 grains, showing that in typhoid fever the stomach loses nearly half its bulk. Nor is there merely loss of bulk, there is also impairment of functional activity, for in seven cases of typhoid fever he made an artificial gastric juice with the mucous membrane of the stomach, and found that the average amount of coagulated albumen was only one grain, while the amount ordinarily dissolved by the same in those dying of other diseases, as phthisis and pneumonia, was four grains. In

two of the typhoid cases the gastric membrane had lost its peptonizing power entirely, so that the albumen was quite unaffected by the artificial gastric juice made from them.

In every case of typhoid fever, therefore, we should bear in mind that we have to deal with a stomach which is reduced to the weakness, or even more than the weakness, for digestion, of the stomach of a newly born infant. Not only, therefore, should we abstain from loading such a viscus with strong animal broth, for several reasons which we will advert to presently, but the administration of that great resource of ours in fever feeding, namely, milk, should be correspondingly regulated, if we would not do more harm with it than good. Milk, while an ideal food in that it alone of all known articles can prevent starvation of any kind, because not a needed element in the body is without its proper representation in it, yet is by no means an easily digested article of food by the healthy adult stomach. We doubt if the majority of healthy adults could be kept so for three weeks on an exclusive diet of good cow's milk: for both gastric and peptic derangements are so common from taking this article without proper preparation of it, that we are continually meeting with patients who tell us that they have found by experience that milk does not agree with them. The reason for this is not far to seek. The necessary coagulation of the casein in the stomach, which is the first step in its digestion, uses up so much of the gastric secretion that not enough of it is left to complete its conversion into peptones, and it is due to this fact that all nomad people who have to live exclusively on milk have discovered that the stomach must be spared this preliminary step in the digestion of milk, just as much as the starchy elements of vegetable food must be first properly softened by boiling or otherwise cooking; and hence whether they be Tartars, or Bedouin Arabs, or South American Guachos, they never take milk until it has been fermented first. The Arab or Turkish form of ferment-

ing milk, to which the names "lebben" and "matzoon" are given, I much prefer to the more acid fermented article of the Tartars, called "kumyss." Lebben is fermented every day from fresh milk by adding to that which has just been brought in from the cows some of the fermented milk left from the previous day, the first fermentation of which originally was made by the yeast plant by means of dough or common yeast. When thus daily prepared it is of but a slightly acid flavor, is smooth like cream and not in large curds, and in my experience has been the most serviceable food that I can find for chronic gastric catarrh, gastric ulcer, and other serious derangements of stomach digestion, and likewise in the treatment of phthisis, Bright's disease, and similar wasting affections. I have never seen such success in the treatment of vomiting in organic gastric disorders, including cancer of the stomach itself, as with this fermented milk. But in typhoid fever, even it will prove to be too "hearty" a food in severe cases, and as a rule I prefer to deal with cow's milk in these patients as I would in managing the case of a bottle-fed infant. So often have I found undiluted milk occasion distress in typhoid that I invariably see to it that it is never so administered to them, but rather diluted one-half with lime water. Lime water has the further recommendation that it is also antiseptic, and repeatedly have I had occasion to note an immediate improvement in the digestive powers of typhoid patients who had been suffering from taking undiluted milk, as soon as it was added in the above-mentioned proportion, or even in still larger proportion in very debilitated cases. In some instances the addition of salt is likewise to be recommended, as in the feeding of infants.

But in the marked failure of the pepsin-making glands in this fever we certainly seem to have a further indication to treat them on the restorative principle of artificially supplying the deficiency of the digestive glands by the free administration of pepsin throughout the whole course of the disease. It has, therefore, been my un-

varying practice for a number of years to administer ten grains of saccharated pepsin every three hours, often, if the tongue be dry, with the dilute muriatic acid, so that from forty to eighty grains are given in the course of twenty-four hours. Any one who has studied cases of chronic diarrhœa will have occasion to note how often the improvement of gastric digestion is followed by improvement in the intestinal disorder, and it is readily appreciable that the passage from the stomach into the bowels of a mass of ill-digested and fermenting food must be prejudicial to its further proper digestion in the intestinal tract. Hence, although pepsin assists only stomach digestion, it is often of great service in the treatment of intestinal disorder, if only, it may be conceived, from its powerful antiseptic properties. With this agent, if properly manufactured, therefore, I think that we have a material aid for the great indication of combating the formidable complication of failure of strength of this typical adynamic affection, instead of waiting for its manifestation and then having recourse to the deceptive properties of stimulants. It is difficult, especially for a beginner in practice, to avoid relying upon stimulants, as if they can really sustain the lessening vitality of fever patients. They seem to hold the patient up, as if in each dose we supplied him with what his blood does not supply him; that is, with a positive addition to his living force when he is in sore need of it. Stimulants are therefore apt to be given frequently and continuously in small doses—doses that can hardly be felt as stimulant by a healthy frame—evidently with the conception that we can thus continuously keep life a-going. One might as well cure the laziness of his horse with mere ticklings with the whip. How much better would it be for the patient if the one dominant idea of the practitioner with reference to this indication was that starvation is the real danger, and that starvation can neither be provided against beforehand, nor adequately dealt with when present, by stimulants, but only by food. My use of stimu-

lants in typhoid is rather to reserve them for what may be termed the emergencies, and these emergencies generally present themselves according to certain physiological laws. Thus in health there is not a uniform point of vitality, so to speak, maintained by the system throughout the twenty-four hours, but rather a regulated succession of periods which contrast with each other in the frequency of respiration, in the force of the pulse, and in the whole tone of the muscular system. The highest point of vital power is about 10 A.M., and the lowest about 2 A.M., and between these two poles the pendulum swings, from the full activity of the forenoon hour to the decidedly lessening strength of evening, until it reaches the positive feebleness of the later hours of the night, when the fall of the last of all functions to vary, namely, the bodily temperature, frequently in good health amounting to over a degree and a half, between 2 and 5 o'clock in the morning, often causes us to awaken and increase our bed covering. These variations are still more strikingly illustrated in disease, as witness the first appearance of night sweats in phthisis, and the most frequent time of onset of looseness of the bowels in chronic diarrhœa. It is, therefore, my practice to direct that stimulants should, at first certainly, be given in typhoid only after midnight, afterwards, as weakness progresses, in the evening, and earlier than this only as the indications are unmistakable for them. When given, also, an ounce every three hours of whiskey is better than twice that amount administered in half-ounce doses every half hour. Meantime the forenoon is the time to push feeding, rather than to increase the administration later on, because the patient then seems weaker. The old reliance on beef tea as a nourishing thing is happily pretty generally abandoned by the profession, but I have a distinct objection to animal broths in typhoid fever, quite apart from the question of their nutritive properties. I have no doubt that in many afebrile conditions of exhaustion they are often very serviceable, if not as foods, at least as promoting the acceptabil-

ity and thus the digestion of other foods. But in the general deficiency of digestive secretions in fevers, and of course notably in typhoid, there is a particular proneness of animal broths to take on, often very rapidly, acid fermentation in the alimentary canal, owing to the absence of those most powerful of antiseptics in the living body, the digestive secretions themselves. I was early led in my practice to give up beef tea, which was then very widely relied upon in the treatment of typhoid, from noting its undoubted effect in increasing both the meteorismus and the diarrhœa; and I have never seen it fail to promote the development of these complications in those cases which I have been called to see in consultation where it had been allowed by their medical attendants; nor, I may add, have I in these same cases failed to note the improvement in these respects which followed upon altering their feeding. I speak of this because recent authorities are again advocating the administration of eggs, broths, and even meat, to typhoid patients. The question here is not whether many patients will not survive a hearty diet in typhoid. Many doubtless will survive, however they are treated, as the great majority of armies survive the worst battles; but the question is simply whether a typhoid patient can be fed as if he were not sick, or only slightly affected with indigestion, so that we may give his stomach just as much food, or as much to do, as if he had no specific process of disease attacking his stomach and intestinal digestive glands.

After every measure has been taken against the great complication of the progressive exhaustion which the slow fever process causes by its interference with the food supply of the body, the next indication is to prevent self-poisoning. Auto-infection is more or less imminent in every fever, because not only are the digestive secretions interfered with and thus blood renewal diminished, but the emunctory secretions are equally disordered, and instead of a constant balance being maintained between the

blood-making and the blood-purifying processes, we have, by the derangement of the latter, a poisonous accumulation of excrementitious matter going on, which soon indicates its effects by the symptoms of toxic infection appearing along with those of increasing debility. Hence, the symptomatology of all fevers, when they have been both severe and prolonged, becomes so much alike and to such a degree resembling the common conditions of this fever that we are accustomed to speak of them in general as then passing into a "typhoid" state. But in typhoid fever itself we are necessarily brought to this condition by its specific connection with the alimentary canal. The part which gastro-intestinal disorder has in producing functional nervous diseases of all kinds, such as mental depression, headaches, neuralgias, and convulsive affections like hysteria and epilepsy, has of late years been so strikingly demonstrated by the progress of physiological chemistry that a great light has thus been thrown on many of the most obscure, and at the same time most important, questions in practical medicine. It is now abundantly proved that at all times during normal digestion there are formed, especially in the intestinal tract, a number of definite animal alkaloidal poisons which are absorbed into the portal circulation, and, after being in great part rendered innocuous in the liver, are also in part sent to the kidneys for excretion, and that it is mainly from the presence of these ingredients that normal urine is so much more poisonous when injected into animals, than the urine which comes from a kidney whose secretory functions have been damaged by chronic Bright's disease. The explanation which this gives of many disturbances of the nervous system from any form of indigestion, but especially when the hepatic functions are much involved, is not only probable, but has been confirmed in my mind by the improved results of treatment of every form of functional nervous diseases from migraine to epilepsy, since I have come to regard them as indicating some form of self-poisoning by animal ptomaïnes or leucomaïnes rather than as proceed-

ing from some fault in nervous organization. In typhoid fever we very early have before us a case of self-poisoning from absorption of intestinal poisons. The specific germ of the disease does not travel beyond the intestinal tract, unless accidentally introduced through some ulceration, but, like most other bacteria, its growth is accompanied by the formation of soluble toxic products which then enter the circulation to produce their characteristic effects. That we can specially neutralize these poisons has not yet been made clear, but there can be no doubt that the great increase in the putrefactive changes in the secretions of the alimentary canal itself, as well as in the materials that are introduced as food and drink, which the local action of the typhoid bacillus promotes, can be very much modified by treatment to a great gain in the chances for recovery. It has been estimated that the quantity of the secretions which are daily poured into the alimentary canal by its apparatus of glands is equal in bulk to the mass of the blood itself, nearly every part of which is reabsorbed into the circulation. This great flow and reflow of secretion, indeed, might be called a second circulation to that of the blood itself, and we can readily see how, in the sojourn of the fluids in the stomach and bowels, every chance may occur for the entrance into the systemic circulation of enough neurotic poisons to produce most symptoms of the disease, both of a nervous and muscular kind, as well as originate a great variety of the familiar sequelæ of this fever. Upon every rational ground, therefore, intestinal antisepsis seems to be indicated in typhoid fever, and the means which may be chosen for this purpose should be carefully studied and tested.

A singular idea has been mooted by some that we have no means of disinfecting the alimentary canal. Overlooking their own practice of clearing out the bowels with castor oil when symptoms of dysentery first begin after some intestinal fermentation, and the universal practice of treating summer bowel complaints by intestinal antiseptics instead of by locking them up with opium, we

hear experiments quoted which are supposed to prove that we have no agents which can modify any putrefactive processes in the bowels. As Brieger and others have shown that such substances as indol, phenol, skatol, and creosol, which are to be found in normal urine, are the products of decomposition going on in the intestine under the action of the micro-organisms of putrefaction which are constantly present there, so Stieff publishes (quoted from the *Journal of Medical Sciences*, December, 1889) a series of observations on patients in Gerhardt's Clinic to whom he administered various supposed intestinal antiseptics, with a view to determine whether he could thereby diminish the percentage of these substances in the urine. His conclusions are: 1. Calomel, given in doses of five grains three times a day, exhibited no disinfecting power in conditions of increased decomposition in the intestine. 2. Consequently it can scarcely be recommended for the purpose of checking putrefaction in the intestine, on account of the large dose which would need to be employed. 3. Camphor appears to possess a slight restraining power on putrefaction, since, in two cases, doses of five grains three times a day produced a diminution of intestinal decomposition. This action of camphor does not appear at once, and is only distinct after two or three days.

The ready answer to such isolated and inconsequential experiments is that it is very unlikely, as it would also be very undesirable, that we can by drugs materially modify one of the most necessary functions of life, viz., normal intestinal decomposition. We might as well expect by drugs materially to decrease the excretion of carbonic acid by the lungs. Natural decompositions are adapted to go on in spite of a great variety of influences which may readily affect pathological or unusual processes; and that we are constantly dealing successfully with fermentations of many kinds in the stomach and in the bowels by remedies whose action can be regarded only as anti-

septic, is too much a matter of daily experience to be doubted by any intelligent physician.

Of all restorers of arrested secretion by the process of fever, a calomel cathartic seems to me by far the best. As above remarked, the onset of fever shows us the signs of universal suspension of secretion, and we have in calomel an agent which, unlike most other eliminatives, operates not on one set of glands but on the general secretory system. The dry skin becomes moist, and therefore with a gentle perspiration we likewise note the disappearance of thirst, and while the bowels are acting the kidneys also for a time increase their secretion. It has further seemed to me that the initial suspension by fever of the excreting functions causes such an accumulation of effete matters in various parts of the body, that a larger amount of pabulum for the growth of the developing specific micro-organisms of the different infecting fevers is thereby produced, and hence we have another indication for an early resort to calomel purgation. In scarlet fever I should be very unwilling to omit this measure, and it is my uniform practice to administer a nightly purgative of three grains in this disease until the tongue desquamates, having been led to adopt this measure from observing the uniform good effects which it causes in mitigating the development of the various complications of its course. Within three months I have thus treated one of the most malignant cases of scarlet fever that have ever occurred in my practice. Within thirty-six hours from the beginning the swelling of the throat was so great that fluids constantly regurgitated through the nose; the accumulation of sordes on the teeth occurred within but a few hours from the beginning, while otitis soon complicated the case. But one of the most ominous symptoms was great meteorismus, soon followed by profuse and exceedingly fetid diarrhœa. I at once checked this diarrhœa by a calomel purge, and as often as it occurred in the following weeks this drug would as promptly change the character of the stools and soon suspend the morbid action. In the begin-

ning of typhoid fever, if the patient is admitted before the tenth day, I give the same cathartic twice or three times a week, and desist only when there seems to be reason to judge that ulceration of Peyer's patches is setting in. The earlier in the disease that this measure is adopted the better, as indeed is the case with treatment in general in typhoid fever, as it is a matter of common experience that those who persist in walking about for a good while before taking to bed with it are apt to have it the more severely afterwards.

My main reliance for continued intestinal antisepsis, however, is on bismuth. I prescribe from forty to eighty grains of the subcarbonate daily, to be given with the same doses of pepsin. Bismuth is one of the best topical antiseptics that we possess, even as a simple surgical dressing; but its chief recommendation for internal administration is its entire freedom from causing local irritation. It is the very reverse of an irritant, and may be used to prevent the topical effects of other drugs which are so. After carefully observing the effects of a great variety of agents recommended for their supposed antiseptic properties, such as naphthalin, naphthol, thymol, salol, resorcin, iodine, and carbolic acid, I am sure that in the tender and inflamed mucous membrane of the stomach and bowels of typhoid fever we must be cautious of administering too continuously any agent which, when taken into a *healthy* stomach, soon causes either uneasiness or is repeatedly tasted again; and so I have experimented with every one of the substances on myself, and, with the exception of resorcin in solution, found the verdict of the stomach to be that it did not want any more of these things, for it took a long time to dispose of them, as was plain from its repeatedly sending back their taste into the mouth. Resorcin, which in my opinion holds the first rank as a remedy for gastritis, yet is too soluble and quickly absorbed by the stomach to be of much service as an intestinal antiseptic. But with free doses of bismuth, statedly administered, one may look for two

results in typhoid fever: the first a diminution of tympanites, and secondly a spontaneous arrest of the diarrhœa. In fact, a typhoid-fever patient with diarrhœa is quite an exception in my wards, and I am scarcely obliged to treat this complication by itself in more than one case in twenty. I think, therefore, that on this account we must ascribe to this treatment a most favorable effect on the intestinal ulceration, for it can be readily imagined that for an ulcer to be constantly bathed in the cauterant, fermented fluids of typhoid diarrhœa, the difficulty of healing must be great. I have of late sometimes employed the salicylate of bismuth instead of the carbonate, but I do not know whether it be better or not. From the good results of the administration of two-grain doses of ichthyol, with bismuth or soda benzoate, in the treatment of the tympanites of women with hysteria or at the menopause, I do not know but that it may also be of service in typhoid.

For many years I have been accustomed to administer turpentine also, whenever the tongue becomes dry, as recommended by the late Prof. Geo. Wood, of Philadelphia, and, as I think, it justifies the encomiums which that experienced practitioner pronounced upon it.

We now come, in conclusion, to the important subject of the so-called antipyretics in typhoid fever. While it may be generally accepted that a continued high range of bodily temperature is of evil augury in this affection, yet the converse of this is not equally true. In beginning my service, in September, 1883, at the Roosevelt Hospital, I was struck with the number of cases in the wards who presented grave constitutional symptoms along with a low range of the thermometer throughout, and in three who died the temperature did not rise above 103° until shortly before death. No one symptom, in fact, should ever be relied upon in our estimation of this disease, although when taken by itself the temperature oftener indicates the degree of the severity of the particular case than any other indication. But the progress of modern

physiology has thrown considerable light on the fever process by its demonstration that the muscular tissue is the true furnace of the body, and that it alone supplies at least five-sixths of the animal heat. Moreover, that the regulation of the heat generated and of the heat dissipated, which in health so wonderfully keeps steady the thermal point at 98.5°, whether on the Arctic Ocean or at Zanzibar, is effected by a special nervous influence emanating from special nerve centres. We must, therefore, look upon muscular tissue as having two entirely distinct functions to perform—namely, that of muscular contraction and that of heat production—and either one of these may be called into activity or paralyzed without the other being involved. This is shown by the fact that the chemical proofs of combustion—namely, the absorption of oxygen and the exhalation of carbonic acid—are manifest quite as much in living muscle when it is at rest as when it is contracting ; and hence, even when a large muscle is at perfect rest, the blood which leaves it by its vein contains more carbonic acid than the blood of the right or venous ventricle of the heart. Dr. McAllister experimented with thermometers introduced between the thigh muscles of a pithed frog, and then found that the heat function could be exhausted by electrifying the sciatic nerve before the contractile power was so, and also that in warm-blooded animals, as rabbits and dogs, he found that, when narcotized and then either artificially heated or cooled, the heat-making function might be paralyzed while the motor function remained. The dependence of both these functions of muscle, however, upon nerve regulation is well shown by the effect of curare, which abolishes both of them, because it arrests the function of the peripheral nerve endings, and thus blocks the way for the transmission of either kind of nerve impulses. The thermal behavior of an animal in whom the heat-generating tissues are thus cut off from the influence of the nervous system is remarkable. For although the vasomotor system yet remains intact, not being affected by

curare, still the animal can no longer regulate its temperature. If plunged into a cooler medium its temperature steadily falls, if into a warmer it similarly rises, and it becomes the plaything of external conditions; and meantime, although its circulation may remain unaffected, yet if cooled its consumption of oxygen and exhalation of carbonic acid falls to a mere fraction of the normal. These facts have led to the discovery of the centres which preside over this great function of life, and have shown how by stimulating them we can artificially induce a condition which in its nature and results cannot be distinguished from a true fever. Aronsohn and Sachs found that puncture with a fine needle, vertically, through the medial side of the corpus striatum, near Nothnagel's *nodus cusorius*, promptly varies the temperature from 2.5° to 4.5° F., and that it remains high for many hours, and also that by stimulating this region with electricity the temperature of the muscles rose, and could be kept so at will for many hours, with a corresponding increase of the oxygen inhaled and the carbonic acid exhaled, and also with an increase of nitrogen excretion just as in fever—all this without encroaching on the motor tract or function, or without calling into play any vaso-motor act. These results have been confirmed in the main also by the experiments of Hitt and of Horatio Wood in this country. We have in these facts, first, a striking confirmation of the truth, learned by clinical experience, that of all tissues the muscular tissue suffers the most severely in fever. Muscular languor is its first note, and then muscular weakness, going on to complete relaxation from general prostration. More and more the heart gives its evidence; and not only so, but it wastes just as every muscle wastes. The indication for muscular rest is, therefore, an early and a late indication in all fevers, and in none more so than in typhoid. Whatever increases muscular weakness, by so much adds more danger, and on that account I would unhesitatingly exclude from the treatment of typhoid fever all members of the so-called antipyretic drugs

which belong to the coal-tar or carbolic-acid class of substances. This would include quinine, which ere long will be made probably from carbolic acid, as its near ally, salicylic acid, is made now ; also antipyrin, antifebrin, phenacetin, and the rest of this rapidly multiplying family of drugs, many of them valuable drugs for other purposes, but not for this purpose. These agents are one and all muscular paralyzers. A large dose of quinine given to a healthy athlete will quickly substitute for the vigorous stride of the legs the weak tremor in them of old age. I have seen the most mischievous results from this by weakening the heart in pneumonia by the former German practice of thirty-grain antipyretic doses of quinine, and while doubtless, by their property of acting on the muscular tissue, they bring down the temperature, yet what have we gained if we bring the patient down, too, along with his temperature? Repeatedly have I noted the effect of these drugs to be that of improving this one symptom of pyrexia, while every other symptom was made apparently worse. In no case should we in our practice fail to note what may be termed the harmony of signs. A single unmistakable note of discord among symptoms is often a true reason of apprehension in one case or equally of hope in another; and hence I would derive but small encouragement from simply a lower reading of the thermometer, when I could not read any signs of betterment in the aspect of the patient. It is true that in the earlier stages of the fever, while the patient has still a considerable reserve of muscular power, these antipyretics often seem to bring down the fever without much detriment, and hence many physicians speak well of their experience with them. But medical experience is a witness whose testimony, however honest, yet needs very careful sifting. Rather than trusting to the observations based upon the administration of these drugs as soon as the temperature begins to rise in the history of the case, I would ask that the effect be noted in these sudden elevations which frequently occur during the decline or even in the slow con-

valescence from typhoid, when the patient is quite weak. A man in the Roosevelt Hospital last October had a tedious recovery from a very severe course of the fever, during which, after a week of normal temperature, he had one night a rise to over 105°. The house physician concluded to try for once the effect of a moderate dose of antifebrin, but, like a good boy, he told me, on my visit the next day, that he would never do so any more, for the man went at once into a state of collapse from which it took hours to restore him. Here the simultaneous effect of antifebrin on temperature, and upon something else besides temperature, was fully illustrated.

Wholly otherwise I must say has been my experience with the employment of the cold bath in typhoid fever. Fifteen years ago, in the year 1875, I published three lectures on the treatment of typhoid fever in the New York *Medical Record*, from which I beg to quote the following short extract on the use of the cold bath:

"Now, what are the effects of this treatment, and how often should we prescribe the dose? In severe cases you will generally note the refrigeration to last about an hour, and then the thermometer commences slowly to rise again, so that by two or three hours it reaches its former point. If so, then I would recur again to the bath every two hours, or twelve baths, if possible, in the twenty-four. In such a case it may be that nothing but such a course will save the patient's life. But much more commonly you will find that you have broken the high range for three or four or six hours, or even for a day, so that you need but four, three, or two baths. The thermometer should be your guide, for you should begin with a resolution that you are not to allow the patient to have a fever above 102° in the morning or 103° in the afternoon, or still lower, according to the nature of the case. Hence, in severe cases, it is just as essential to give the baths during the night as during the day, and you will never have cause to regret your pertinacity in so following up the pyrexia for the whole time. The symptoms of the patient will soon

show you that you are doing well by your perseverance. After the bath he is a great deal quieter, his delirium in nearly every instance improves or subsides altogether, to return only with the renewed rise of the thermometer. The breathing becomes easy and natural, the pulse improves, and often the sufferer drops into a quiet sleep accompanied by a gentle perspiration. But what is of special significance is that the course of the thermometric range of the fever itself in the majority of patients becomes permanently lowered to a point at which the chief perils of the disease will hardly be met with. You seem to be pursuing a different road from the well-known track of typhoid fever. Instead of those numerous features, such as clouded intellect, muttering delirium, subsultus, etc., your cases will often remain with but little change from day to day, during the second and third weeks, from their symptoms at the end of the first week. . . . But another great gain is that food can be better taken, because the digestive glands are able to resume their work. A good indication of the benefit of cold baths is often manifested in the natural state of the tongue and mouth secretions. Instead of the dry, cracked, bleeding, and brown tongue, and the sordes collected on the teeth and lips, I have frequently seen the tongue, even in severe cases, remain nearly natural during the whole course of the complaint."

The experience above given I have not had reason to doubt by the further experience of fifteen years of hospital service during the months in which typhoid fever most prevails in New York. The procedure which I have followed is almost identical with the directions laid down by Brand, except that he lays great stress on friction of the surface of the body of the patient with the outstretched palm of the hand while in the bath, as a means of preventing collapse and also of promoting further dissipation of heat. The official reports of the Second German Army Corps of the reduction of mortality in typhoid fever by this treatment are so extraordinary that, were it not for

the well-known accuracy of these reports, it would be difficult to credit them. From a mortality of 26.3 per cent before adopting the Brand treatment, the figures record only 4.3 per cent by systematic bathing. Other statistics by such clinicians as Juergensen and Ziemssen claim to be even more favorable, and the least that can be said of them is that no claims of success at all approaching them in treatment have ever been advanced by as trustworthy observers as they are. The general theory advocated by Brand and his supporters is that the cold bath acts as a stimulant upon the nerve centres implicated in the fever process, and, therefore, in an opposite fashion to the antipyretic, sedative operation of the coal-tar series of drugs. I am now inclined fully to indorse that view. In 1875, under the influence of Liebermeister's teaching, I recommended antipyretic doses of quinine as an adjunct of the cold bath, but I soon gave that up on noting a decided gain by relying on the baths alone. Many recent advocates of Brand's method, however, as might be expected, claim the whole earth and more for their favorite system. One would suppose from their statements that the bacillus of typhoid in the intestine is itself drowned by the cold water on the outside of the patient's body, and that we need not trouble ourselves with much care about feeding, and none about intestinal antisepsis or any other indication in typhoid fever. But if the cold bath acts as a neurotic, and acts just the same on all patients, then it is the only neurotic known that does so, for nothing is so common with agents that affect nerve functions as to meet with personal idiosyncrasies in which the effects are wholly different from those which are met in the majority of other persons. I have found just the same difference in the effects of cold baths in typhoid patients. With some, no matter how carefully administered, the effect has been the reverse of stimulant, and I have had to give them up; but they have been but few in comparison with the undoubted advantage which a systematic and early recourse to this measure has shown in what I may now claim as a fairly extensive experience.

A REFORM IN ARTIFICIAL INFANT-FEEDING,

WITH AN IMPROVED AND INEXPENSIVE STERILIZER AND NURSING BOTTLES.*

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ALL food entering the body above its physiological quantities is useless for nutrition, a hindrance to digestion, and a danger to health. This fact, known to the medical men of all countries, so important to the welfare of all, and particularly to that of the bottle-fed babies, has been disregarded to such an extent that it really seems surprising to find that, while animated discussions have been carried on for decades in medical journals and text books regarding the proportion of milk and gruel, casein, sugar, and fat, and nearly every authority has given to the world his own proportions, not until lately have any of them thought it important enough to give precise data concerning the quality of food to be taken. Gerhardt (in his text book on "Diseases of Children," 1887) does not even mention the word quantity in his chapter on nutrition. Jacobi (in his book on "Intestinal Diseases of Infancy and Childhood," 1887) makes the following statements concerning the quantity of food to be given to infants: "Practically it is not very difficult to regulate the quantity of nourishment taken. *Healthy children will fix the proper limit themselves.* The same rule will apply, with slight differences, in an equally good condition of health, for the nourishment artificially prepared. But it

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* Read before The New York Academy of Medicine, January 16th, 1890.

is impossible to lay down as a uniform rule that some particular quantity must be considered as normal for all children." "*The child ought to drink from the breast or bottle until it has had enough.*"

This state of affairs has left a wide field for every practitioner, midwife, and mother to individualize, and the result has been that, almost without exception, every artificially fed infant has been overfed. The ordinary nursing bottle contains six or eight ounces at least, and this quantity was and is given to the new-born as well as to the three-months-old baby, and when the younger begins to regurgitate food, becomes dyspeptic and constipated, and even loses flesh, the comparative merits of the proportion of fat, sugar, and casein are considered, and the different methods of the authors are tried, one after the other. At last some patent food is resorted to, and, as the weak stomach finds it less laborious to overcome the large quantity of this pseudo-food than that of diluted milk, the mother and the doctor are once more convinced that this patent food has saved the life of the baby.

Biedert (in the ninth edition of Vogel's text book, 1887) makes this statement: "The amount of food regulates itself so that food is never given oftener than every two hours; and if the child keeps quiet, wait still longer, and give less if the child vomits, and more if it shows ravenous hunger."

Escherich was the first to call attention to this deficiency in precise regulations as to the quantities in infant-feeding. In a table arranged to correspond with the tables and figures of Pfeiffer (Wiesbaden), who determined the quantities of milk taken by nursing infants by weighing his own baby before and after nursing during a whole year, Escherich gives precise figures as to quantity and proportionate dilution of the milk according to the *age* of the children. His volumetric method of artificial infant-feeding (*Wien. klin. Woch.*, No. 40, 1889) is certainly an improvement, and if the bottle devised by him, showing the exact amounts and proportions of milk and

water, would be used as he has directed, not alone according to the months but even the weeks of life the child has existed, no doubt many little ones would be all the better off for it. But his method is an ideal one, and, therefore, deals with ideal mothers, who will put just so much food in the bottle large enough to hold much more, no matter how much their offspring clamor for more, and will never think that a "child ought to know best when it has had enough," and never act accordingly; and he deals with ideal children, all of one size and one weight, who always gain in flesh and in proportions of stomach capacity according to the rules laid down in Gerhardt's text book on "Diseases of Children," Vierordt's "Physiology," and Pfeiffer's tables, never varying in the least.

But mothers *will* give their infants more food without hesitation, if they only notice that the youngster keeps on sucking its finger after the bottle is empty, and especially if a large bottle is given them to feed with; and the infants will *not* invariably increase their digestive power and the capacity of their stomachs according to text books and tables; they will even be born with different-sized stomachs, and will not gain in flesh and weight, as Escherich seems to believe, but will arrange that entirely according to circumstances coming up during these twelve months of life—circumstances that either promote or retard the digestive ability, size of the stomach, and weight of the child. Only one thing really does keep on increasing steadily, and that is the age of the infant. Escherich makes the same error that Fleischman and Ahlfeld committed when both weighed children of the same age to determine the amount of food taken by them. Their results were far from even, which led Jacobi ("Intest. Diseases," p. 86) to the following sarcasm: "Can there be anything more exact? Both forget that the nursing, which occupies a child fifteen to thirty minutes, represents no invariable quantity, and that the whole quantity cannot be found at any time entirely in the stomach."

Directions.—Weigh the naked baby, and then find the amount and composition of its food and the manner of feeding, in the table.

HOW TO FEED AN INFANT. Devised by A. Seibert, M.D.

Weight in pounds.	Number of bottle.	Size of bottle.	Amount			Time of feeding.			
			Of milk.	Of gruel.	Of sugar.	How often.	In 24 hours.	From 6 A.M. to 6 P.M.	From 6 P.M. to 6 A.M.
6, 7, and 8.	I.	3 ounces.	1 ounce, or 2 tablespoonfuls.	2 ounces, or 4 tablespoonfuls.	half-teaspoonful.	1 bottleful every 2 hours.	8 bottles	6 bottles.	2 bottles.
9 and 10.	II.	4 ounces.	1½ ounces, or 3 tablespoonfuls.	2½ ounces, or 5 tablespoonfuls.	half-teaspoonful.	1 bottleful every 2 hours.	8 bottles.	6 bottles.	2 bottles.
11, 12, 13, and 14.	III.	5 ounces.	2½ ounces, or 5 tablespoonfuls.	2½ ounces, or 5 tablespoonfuls.	three-quarters of a teaspoonful.	1 bottleful every 2½ hours.	7 bottles.	5 bottles.	2 bottles.
15 and 16.	IV.	6 ounces.	3½ ounces, or 7 tablespoonfuls.	2½ ounces, or 5 tablespoonfuls.	three-quarters of a teaspoonful.	1 bottleful every 2½ hours.	7 bottles.	5 bottles.	2 bottles.
17 and 18.	V.	7 ounces.	5 ounces, or 10 tablespoonfuls.	2 ounces, or 4 tablespoonfuls.	1 teaspoonful.	1 bottleful every 3 hours.	6 bottles.	5 bottles.	1 bottle.
19 and 20.	VI.	8 ounces.	All milk and 1 teaspoonful of sugar.			1 bottleful every 3 hours.	6 bottles	5 bottles.	1 bottle.

The *weight*, not the age, of the infant determines its food properly.

Never use a larger bottle than the one indicated by the child s weight.

In venturing to answer this question of the father of modern infant-feeding, I hope not to be deemed impertinent when I say that there is something more exact, and these experimenters did not succeed, and could not succeed, because they weighed out the stomach capacity of babies of one age and not of one *size* and *weight!* This is not exact and never will be, *for the weight and size of infants of the same age vary as do their faces, and hence necessarily the capacity of their stomachs also.*

For the last two years I have paid more attention to the stomach capacity and digestive power of infants than formerly, and seeing large numbers of children run down by disease, retarded in growth by improper food and feeding, or naturally small by heredity, and, on the other hand, children above the usual size and weight, and yet of one age, it naturally occurred to me that it would be entirely improper to regulate their food so as to give them all the same quantity. And so I took to weighing these infants, and was at first astonished to find them vary so much in weight. Thus, it has been nothing unusual to see two infants, in the same hour at my clinic, corresponding in age, and varying in weight so as to show the one to have fourteen and the other but seven pounds of body weight. The second conclusion I came to, after having weighed a greater number of infants, was that it was quite easy to determine the bulk of food a child could take, or, better, ought to take. The success of arranging the feeding in this manner was very gratifying, and so gradually I was able to arrange a table in my mind similar to the one you have in your hand, which has since guided me in determining the quantity and composition of food. Since October, 1888, I have frequently written out this table on the blackboard in the Children's Department of the New York Polyclinic for the benefit of my class; so in reality this is not a new method with me.

It would have hardly occurred to me that others did not, to an extent at least, work in the same direction, had it not been for one of the latest articles of Escherich

(*Wien. klin. Woch.*, No. 40, 1889), in which he pleads for reform in artificial infant-feeding in reference to the amount of food. As mentioned before, he adheres to the method of feeding according to the *age* of the children. Escherich does not even mention the fact that the capacity of the infantile stomach varies very considerably in children of the same age. On a closer inspection of recent writings on infant-feeding, I found that Biedert (in Vogel's text book, 1887) remarks: "In case the digestion is very easily disturbed, a more exact regulation of diet would be necessary, so as to give two hundred cubic centimetres of properly diluted milk to every kilo of the child's weight." Rotch, in the August number of the *Archives of Pædiatrics*, 1889, thus calls attention to the different sizes of infantile stomachs: "Of the different causes which regulate the gastric capacity, the *weight* of the infant has the greatest influence, and it is perfectly possible for a poorly developed infant of small weight to have a gastric capacity no greater than a normally developed infant of half the age."

These few remarks made by Biedert and Rotch are all I could find bearing directly on our subject. The determination of the bulk of food by weighing infants before and after nursing was practised long ago, but no one has been so exact as Pfeiffer, of Wiesbaden (*Jahrb. f. Kinderheilk.*), with his own child. But in arranging our table we could not go by this one case, as it was that of a healthy child of medium weight. The figures I present in this table are the result of careful notes on about two hundred different children. The manner of obtaining the figures was simple enough. After weighing the child, the composition and bulk of food were ordered, the latter usually being considerably smaller than before. If regurgitation of food, constipation, and dilatation of the stomach and the intestines, with restlessness at night and occasional wind colic, would not disappear, then the bulk and strength of the food were diminished still more until the apparent normal boundary line was reached. This was

done in all cases. The increase in the food was not ordered until increase in weight had previously indicated it. Whenever the bulk and strength of the milk mixture were increased too rapidly, the infant would invariably indicate this by regurgitating the superfluous amount.

Handled in this manner, I have often had the pleasure of seeing infants get well and thrive on three ounces of food that had heretofore been stuffed and starved with five ounces of diluted milk, sterilized and often peptonized in the most approved style. I am at a loss to see why the dilatation of the stomach in infants, which indicates the true condition of affairs in every case as distinctly as the Indian figure shows the cigar store and the red-white-and-blue pole the shop of the hair cutter, has been, is, and probably will be for some time to come, overlooked by the medical attendant. No doubt Escherich is right when he makes the statement that insufficient training of medical students regarding even the very fundamental principles of pædiatrics has been the chief cause of the slow progress of this branch of medicine, and the sceptic and conservative feeling among general practitioners regarding anything new, and, in particular, any tables and charts.

My figures do not alone pertain to healthy but also to convalescent children. If an infant ten months old has been reduced to seven pounds by ailments of any kind, but three ounces of food at the utmost are indicated. If you give more, that baby will vomit, and, at best, not pick up nearly so quick as if handled properly.

On the other hand, a child weighing ten pounds at birth ought to have more and stronger food than one weighing three to four pounds less. The talk about some children of the same size eating more than others is but talk. It takes no more food in one body to keep up the same functions than in another, *provided these bodies are of the same size and do the same amount of work.* Infants do about the same amount of work, so their weight is the only guide left.

I have nothing more to add regarding this method of determining the amount and composition of food for artificially fed children. It is only a new method in detailing and specifying what the most conscientious authorities in pædiatrics have long ago emphasized, and over and over again urged upon the minds of medical men. No doubt my table is deficient, and others will sooner or later make changes, and necessarily it will in each case be left to the physician to individualize; but, on the whole, I do think that this reducing artificial infant-feeding to "facts and figures" will prove a step in the right direction (as Dr. A. Jacobi has been kind enough to call it), and may serve as a practical medium in impressing physicians and parents with the important necessity of not giving infants more food than their stomachs will hold, and of only giving such mixtures as can be digested and assimilated with ease and benefit.

To practically carry out this idea, I deemed it necessary to at first do away with all of the nursing bottles now in use, including not alone the well-known flat arrangement with the long rubber tube, invented only for promoting the laziness of mothers, but also the Soxhlet bottle, which is entirely too large for the new-born and entirely too small for children weighing more than fourteen pounds. Escherich had this same notion exactly at the same time (his article on his new apparatus arriving here while my sample bottles were being made), but he designed large nursing bottles, graduated by marks. For reasons given before, I did not deem it wise to furnish mothers with larger-sized bottles than the size and weight of their offspring indicate, and therefore I designed *six different sizes of bottles*, ranging from three to eight ounces.

Following up the idea of not leaving any part of this feeding method to the judgment of mothers and nurses, I had the bottles properly marked so as to show on the one side the precise mark up to which milk should be filled in, and the word "milk" impressed below this mark. This

simple arrangement permits the omission of first pouring the milk into measures, as designated by Soxhlet and Escherich, or of using a tablespoon of doubtful cleanliness. On the other side of the bottles the direction will be found: For child weighing (so and so many) pounds.

These specifications may seem unnecessary to some, but I believe that when Goethe in his "Faust" made the remark, "For all that you have *black on white*, contentedly may carry home," he once more proved himself to be one of the best judges of human nature. If a proper-sized bottle is placed in the hands of a mother, if the exact amount of milk and gruel is marked and written upon that bottle, and, besides, that it must only be used for a child of a certain size or weight, the chances are that this mother will more readily follow these directions than if they were not there, "black on white," warning her constantly not to disobey this written rule.

My next object in view necessarily was to design the bottles so that they could be easily cleaned. This was simple enough. I merely did away with the long neck, so that, with a pencil, for instance, we could easily touch any part of the interior of the bottles. The neck I had so constructed as to leave not a particle of space between the stopper and its glass surrounding—a decided deficiency in the Soxhlet bottle, which leaves a furrow of sufficient size to retain a portion of the milk, which, after sterilizing, must invariably decompose later in the day, especially in warm weather, and while the child is drinking must necessarily reinfect the milk passing over it before it enters the mouth and stomach of the infant.

The question of a proper and cheap way of closing the sterilized bottles had, up to date, not been solved, as Escherich wrote but a few weeks ago. The stopper designed by me answers all purposes well. It is simple, practical, and cheap. The furrow in its lower third allows the steam to escape during the process of sterilizing, and by a simple turn the bottle is closed air-tight. This is not alone an improvement on the Soxhlet stoppers on

account of being less expensive, but because the aseptic condition of the food is more perfect, as, after boiling twenty minutes, no glass stoppers are put into the bottle, which, handled by the unclean hands of the mother, are brought in direct contact with the food again, thus creating a possible danger for reinfecting the food and harming the child. The ten minutes of further boiling after the glass stoppers are pressed in (during which time they often fly out and fall on the stove or even the floor) is too short a time to kill any germs that undoubtedly have entered the bottle, as Hüppe ("Arbeiten aus dem Reichsgesundheitsamt") showed as early as 1884.

The tray I designed holds the bottles in position and permits the steam to surround them on all sides during the process of sterilization. This tray is so constructed as to be movable and to be fitted for all sizes of these bottles. The holes in its lower part are so constructed that the steam courses up directly in close proximity to each bottle; but its chief virtue lies in the fact that it may be placed in any ordinary pot used for cooking in every household, thus again doing away with a greater expense and another object for care and work for the mother.

My method of sterilizing is the following: After the bottles are properly filled, the rubber stoppers (which fit all sizes) are put into the openings so as to leave the upper end of the furrows or gutters just above the rim of the bottle, allowing steam to escape. Then two cupfuls of water are put into the pot (or the Soxhlet tin pail or the Arnold cooker), the lid is placed on the pot, and the latter placed over a brisk fire. Within five minutes steam appears under the lid of the pot, and, counting from its appearance, the pot is left in position thirty minutes (in cool weather, from November 1st to May 1st). The pot is then removed, the tray is taken from the pot, and the stoppers are turned tightly into the bottles, and the food is safely sterilized.

Hüppe, in 1884, showed that steam sterilizing was far more effective than that done by boiling in water.

My tests I made with milk thus handled proved this to be correct. In warm weather (from May 1st to November 1st) I deem it necessary to steam fully forty-five minutes, as the milk brought then to the houses of city people is in a far more advanced stage of decomposition than in winter time (see my report on "Cholera Infantum and the Weather," *Med. Record*, March 24th, 1888). To do this it is necessary to use three instead of two cupfuls of water for steaming.

The warming of the bottles is also done by steaming. A circular piece of wood or pasteboard, large enough to cover any ordinary tin pint or quart measure (as used in every household), is cut in so as to have an opening wide enough to admit the neck of the bottle to the centre of the board, so the bottle is suspended in the tin hanging over the water. A small quantity of water is put in the tin, this placed on the gas, oil, or ordinary stove, and in exactly a minute and a half the bottle is warm enough for drinking. Any one who, like myself, has had any personal experience in getting up at night and "warming the bottle for the baby," will appreciate this simple device, for it takes fully fifteen minutes to do this according to Soxhlet's method. But this steaming is not alone safer for the food and child, but also safer for the bottles, as there is no chance at all for cracking them by placing them suddenly into hot water, as so far has always been done by cooks, nurses, and mothers, to accelerate the proceedings.

It is my plan to have all druggists in the populous districts sell this apparatus, and to have them weigh the children for the inhabitants of the tenements, and accordingly to determine the exact size of bottles needed in each case. The arrangement is made that the druggists will take back the smaller bottles and give the next size when indicated, with an additional charge of about twenty-five cents.

This brings us to the price of this apparatus. With a written sigh, Escherich exclaims in his last article that the Soxhlet apparatus is too expensive, and, therefore, does not reach those that need it most—the poorer classes. My

apparatus—which, in every respect but in the fundamental principle of sterilizing in bottles, is new and decidedly more exact and less troublesome to handle—has been devised for the special purpose of becoming the infant-feeder for the laboring classes, among whom infant mortality in summer time has baffled the skill of medical men and health authorities for ages back.

It is Soxhlet's idea to sterilize milk in small bottles for infant-feeding, and his apparatus has done much good in preventing infants getting sick and dying of milk poisoning; but it costs five dollars, and, therefore, is entirely too expensive for the working classes, not to speak of the outlay of frequently buying new bottles to replace those cracked during boiling in the water bath. It is patented in Germany, and has been imitated and is sold at the same figure here, though nothing is paid to the patentee.

Escherich's improved sterilizer has been spoken of, and will be hardly less expensive. His bottles are large and graded by lines, and, to my mind, can hardly be used with success by any but very careful mothers. He closes his bottles with the nipples or with cotton (not absorbent)—a manner unfit for practical and ordinary handling.

The Arnold cooker leaves the choice of bottles to the mother, and in that respect must be called a backward step, though the steaming is an improvement.

The six-ounce bottles, filled with sterilized milk, that have lately been brought in the market here, are entirely too large for infants and ought not to be used for infant-feeding, aside from the fact that their price would only suit very rich people, who pay $1.20 each day for this food.

Following the advice of Prof. A. Jacobi and Prof. J. Lewis Smith, who have both been kind enough to inspect my sample apparatus, the firm of Eimer & Amend (corner of Eighteenth street and Third avenue) have cut down the price of my apparatus, so that it can be retailed all over the land at $1. It includes the tray, eight bottles with stoppers, and two nipples.

It is not patented and will not be patented; and if imitated abroad, as I sincerely hope it will be, it will push its way, naked and pure, for what it is worth, as a present to the children of the poor man all over the world, from a member of the New York Academy of Medicine, which deems it improper to patent articles used for the prevention and cure of disease.

Directions for Steaming.—Fill the bottles up to the mark with pure, fresh-bottled cow's milk, obtained from a milkman and not from a store, early in the morning, and above the mark with barley or oatmeal gruel, or plain fresh water. Then add sugar. After filling the bottles as directed, place them in the tray; then put rubber stoppers on them, so as to leave a *small opening at the side of the stopper;* then put two to three teacupfuls of water in any clean pot ten inches wide and eight inches high, place the tray in it, cover it with the lid, and place it over a strong fire. In *cool* weather (November 1st to May 1st) steam fully *thirty minutes;* in *warm* weather (May 1st to November 1st) steam the bottles *fully forty-five minutes.* During steaming the lid of the pot remains closed. Then remove the pot from the fire, uncover it, and *turn stoppers tight into* the bottles. *Never* open a bottle until feeding-time. Bottles are best kept in a cool, dry place.

Directions for Warming a Bottle at Feeding-time.—Pour four tablespoonfuls of water into an ordinary tin quart measure; then put the neck of the closed bottle through a round piece of pasteboard or paper, big enough to cover the opening of the tin; place the perforated half of a blacking-box in the tin so as to serve as a second bottom; loosen the stopper, place the tin on the stove, and steam for two minutes; then remove the stopper and put the nipple on the bottle.

Caution.—Never put the bottles in the water on the bottom of the tin, as they will crack instantly.

GENERAL HISTORY OF THE EPIDEMIC OF INFLUENZA.*

By A. JACOBI, M.D.,

NEW YORK.

Mr. President: The time at our disposal having been divided by you among several Fellows, I shall confine my remarks to but two subjects connected with the epidemic influenza of the last month, viz., its character when studied in infants and children, and the treatment of the most dangerous features of the very worst cases.

Infants and children, like very old people, were rather more exempt than those between the fifteenth, or perhaps twentieth, and the fortieth year. It was not always so, for the history of some of the epidemics emphasizes the fact that in them advanced age suffered considerably. I am not prepared to suggest a reason for the comparative immunity of the young; perhaps it is that they have been less exposed to the inclemency or variations of the weather. The temperatures of the little patients were often but moderate, and quite the reverse of what we are accustomed to observe in all feverish diseases at that age. Still, our observations are subject to mistakes because of the efficacy of our modern antipyretics, which have become too ready instruments in the hands of the anxious practitioner. This circumstance may also account for the absence of the abrupt defervescence and critical symptoms in general, so common in the influenza of the adult.

The usual symptoms of infectious diseases, such as las-

* Read before the New York Academy of Medicine, February 6th, 1890, to open a discussion upon Epidemic Influenza.

situde, muscular pain, sleeplessness, delirium, dry heat or perspiration, suddenness of the attack, a period of incubation seldom amounting to, or more than, one or two days, and anorexia, are met with in influenza of all ages. The local symptoms of the mucous membranes are very extensive. In a common catarrh they are limited to a few; in pertussis they are still more circumscribed locally; in influenza the respiratory organs, from the nose and frontal cavities to the bronchioles; the digestive, from the oral cavity to the rectum, are apt to suffer. According to the more exposed or stricken locality, the symptoms may vary in individual cases; there may be chill or heat, vomiting, constipation or diarrhœa, presence or absence of cough, and dry or secreting Schneiderian membrane.

Nasal discharges were seldom copious, still more rarely tinged with blood. Hoarseness and croupy cough were but rare. Intense laryngitis, such as required tracheotomy in a few of the Russian cases which have been reported, I have not seen. Bronchitis with rhonchi of every description was frequent; the paucity of visible expectoration is explained by the difficulty young children have in bringing up the discharged material, which is swallowed. The bronchial catarrh was very general, and extended over every region of the lung, sometimes with apparently but little suffering. Still I have seen, in children of six years and upward, considerable dyspnœa with slight cyanosis. The frequency of extensive bronchitis furnishes the explanation of how it happened that the pneumonias observed during influenza were genuine croupous, in less numbers than usual. I estimate that of a hundred pneumonias of all periods of childhood, one-third are of the genuine lobar variety; during the three or four weeks, however, in which I saw influenza-pneumonia, I have not seen a single uncomplicated case of that variety. Long after the rational symptoms pointed to the presence of pneumonia it was impossible to discover bronchial respiration, or even bronchophony, which is detected so much more readily; dulness developed very slowly, but

was mostly quite extensive; older children would now and then expectorate small quantities of blood, sometimes of a more or less bright color, or mixed with thin mucus, but never rusty. Extensive dulness was often over the upper lobe, particularly when there had been much cough —a good illustration of Dr. Melltzer's theory of the origin of pneumonia detailed in the *Medical Record.* Sometimes the dulness was pleural; in a few cases the fluid removed by explorative puncture was serous, but rather turbid; finally, however, it was absorbed. In one case only it was so turbid, containing pus cells in small quantities, that aspiration was resorted to. From the left pleural cavity of that child, a girl of less than two years, a few ounces were removed, with convalescence following.

Infants under a year did not suffer much from the respiratory symptoms of influenza. It is only from the second year that these are liable to appear in large numbers; before that the locality of least resistance is the alimentary tract. Pharyngitis and amygdalitis were sometimes absent in the beginning, or but little marked, even in cases of otherwise intense suffering and great prostration. Indeed, the injected throats, so prevalent during an epidemic of scarlatina or diphtheria, were not universal in uncomplicated influenza. Follicular amygdalitis I have seen but once in such a case, without being able to point to an actual connection between the two. When the whole oral cavity suffered it was generally the tongue that began to be affected. The edges became red, the papillæ elevated, even the gums slightly congested and of looser texture. In accordance with the mild affection of the throat, swallowing was rather easy, with the exception of those cases in which there was much rhinitis and consecutive conjunctivitis. The latter, I may here be permitted to state, I never met as the first symptom; that must have happened to the author who placed the location of invasion in that organ. Otitis media I have met with three times, in one case on both sides. The four tympana ruptured spontaneously, and I am told that all are doing

well. Vomiting in the beginning was quite frequent, loss of appetite the rule, diarrhœa not uncommon; rectal catarrh with tenesmus I saw once only.

The scarlatinous eruption mentioned by many I met with but twice: once over the chest only, in a child of two years, with but a slight elevation of temperature; once over the whole surface, with a temperature of 104° and more. I took it for the general erythema liable to appear in predisposed children during any fever. Facial herpes, mostly on the lips, I met several times; urticaria, though it be common enough at that age, not a single time. In the case of serious broncho-pneumonia in a girl of five years, I was told that there had been urticaria ten days previously. It may be that there was a peculiar tendency to exudation, giving rise to urticaria first and pneumonia afterward, depending on either the structure of the blood vessels or on vaso-motor influence. Beaumetz's theory, that the presence of eruptions is a characteristic symptom distinguishing this year's influenza from the common form of catarrhal fever, I cannot share. The nervous symptoms were those common to all feverish diseases. Vomiting was probably due to gastric catarrh, and not to cerebral irritation. Headache was frequent, almost the rule; diagnosticable in infants by the vertical contractions of the frontal muscles and the dazed appearance of the patient; it was a frequent complaint in children of from six to twelve years. A convulsion occurred in a child of three years whom I saw but once, ten days afterward, for pleuro-broncho-pneumonia; very severe intercostal and abdominal neuralgia in a boy of six, with pleurisy of the right side; rigid contraction of the muscles of the neck occurred in several infants, and lasted from one to three days. Temporary delirium was present in a few children with high fever. Paralysis, mentioned by Da Costa, I have not seen. Pain in the extremities was frequent; older children would complain like adults; infants moan or scream, and object to being moved. A tall girl of five, who was reported to be a chronic sufferer

from growing-pain, had excessive pain about the knee and ankle joints. Passive motions did not add much to her suffering, but the slightest pressure on the epiphyses provoked intense screaming spells. Evidently her chronic epiphysitis had met with an exacerbation during her influenza. One girl of three years I was called to see for articular rheumatism ; the synovial membranes and cavity of the sensitive knee were intact, but the intense pain of the epiphyses of the two femora and tibiæ rendered the diagnosis easy. Cold applications and immobilization proved beneficial in a brief time.

The weekly reports of the Health Department furnish the average number (in seasons of average severity) of cases of scarlatina and diphtheria. The comparative absence, however, of measles, and also pertussis, is striking. The two latter are frequently contemporaneous, or precede each other. Their absence during the epidemic of influenza may be accidental, but future research will have to teach us whether it is more than a mere casualty. Vienna journals report the diminution of measles *pari passu* with the invasion of influenza.

Relapses of influenza were certainly not so frequent and marked in the young as in the adult, perhaps for several reasons. The disease was often milder than in advanced age, with a tendency to last longer and terminate in more gradual recovery. But the main reason for relapses was probably wanting, that is, errors of diet committed by the adult, and premature exertion and exposure.

Individual prevention consists in protection against exposure. The skin can be fortified against the results of sudden changes in the temperature by the habitual daily use of cold washing, sponging, or bathing. Catarrhal affections can be avoided by equable temperature of the room and warm clothing. It is true, however, that these measures are more serviceable in common catarrh than effective against the epidemic invasion of influenza. Still, the increased vigor of the superficial integument renders the

internal integument, the mucous membrane, more enduring.

If influenza were a miasmatic disease only, no caution would protect. Among those who claim influenza to be a miasmatic disease only, is August Hirsch, than whom there is no greater authority on the history and geography of maladies. His main reason for believing the disease to be miasmatic is its brief duration. But the number of those who take it to be more or less contagious is by no means small. Of the latest authors I mention only Guiteras (*Medical Record*, January 25th), Nothnagel, and Fürbringer who compares the contagious nature of influenza with that of typhoid fever.

There can be no doubt as to the miasmatic origin of influenza in many well-authenticated instances. Thomas Watson, in his "Lectures on the Principles and Practice of Physic," London, 1871, vol. ii., page 46, refers to the case of the frigate *Stag*. After having arrived near the coast of Devonshire, during a head wind, on April 3d, 1833, she had forty sick within half an hour, sixty more in two more hours, and one hundred and sixty on the following day. But the dissemination of miasmatic germs has its limits. We can hardly suppose that they can be carried thousands of miles through air currents; and the assumption of a proliferation in the air itself is contrary to everything we know of the biology and increase of disease germs. Thus, even when we assume that some stray germs may be saved from destruction in their various travels, we cannot believe that all at once they should so multiply as to infect hundreds and thousands in almost an incredibly short time, particularly in the winter and on a snow-bound coast.

To explain this rapid multiplication we have to fall back on the aid of either soil or the human body. But influenza is found on every soil, normal or frozen, dry or moist, and not every soil suits every germ in the same manner. Thus, it follows that the human body and communication between human beings furnish a readier

explanation for the rapid spreading of the disease. E. A. Parkes, in J. Russell Reynold's "System of Medicine," vol. i., refers to Haygarth and Falconer, in the last century, who traced a number of cases to direct contact. During the height of an epidemic in large cities it is impossible to prove that, but in the beginning of an epidemic, and in the scantily populated districts of the country, such proof can be more easily furnished. The short duration of incubation is no reason for denying contagion, for scarlatina yields numberless examples of the same experience. Bäumler* relates that the first two cases in the surgical clinic of Freiburg were due to a lady lately arrived from Paris. In the hospital the patients in opposite beds caught the disease first. Thus it appears that, while influenza is a miasmatic disease, it is also intensely contagious, and with very short incubation. The fact that in a house full of influenza patients one or more may escape does not militate against the contagious nature of the disease, for the same observation is frequently made during the epidemics of scarlatina, measles, and diphtheria.

Therefore, in the interest of prevention, the attempt at isolation of the sick or the well is justifiable, though the volatile nature of the pathogenous influence is so marked as to render quarantine mostly futile. The immunity, often experienced, of monasteries and penitentiaries justifies the non-admission of influenza patients to public hospitals as far as possible. Thus it was that I advised against their reception in the Mount Sinai Hospital. It is true, though, that serious sequelæ or complications cannot possibly be excluded nor the invasion completely avoided. The question whether the schools ought to be closed during an epidemic is a very serious one; perhaps the consideration that children do not suffer to the same degree with adults militates against the enforcement of such a measure. In Vienna, a few weeks ago, the Christmas vacations, just entered upon at the outbreak of the epi-

* Bäumler in Münch. med. Woch., 1890, No. 2.

demic, were lengthened. If the schools cannot be entirely closed, care ought to be taken that they be aired more carefully than ever, thoroughly heated through the night, and an equable temperature, measured more by the thermometer than by the individual pleasure, wants, or sensations of the teacher, kept up all day.

Though influenza be a self-limited disease, it must be treated. The discomfort it carries with it, the prostration it produces, the sequelæ which render its invasion a possible or probable danger, require relief. Statistics prove nothing when it is stated (as, for instance, in Berlin) that no death has occurred of influenza. Statistics prove a great deal when we remember that a few weeks ago our death rate was doubled. There is no better instance to prove that not the malady but the sick must be treated. Treatment will not shorten the disease, but it may save strength and facilitate and shorten convalescence. The patient must keep in bed during the illness and part of the convalescence. Whoever does not, risks a relapse. Eyes and brain are rested by excluding bright light and noise. Muscular pain and fever can be soothed by diaphoretics—acetate of ammonium, aconite; cough by opiates; intestinal stagnation and gastric discomfort by a single purgative dose of calomel administered at once; very copious secretion by belladonna. Beverages must not be iced ; food fluid.

All the known old and modern antipyretics have been used. Quinine appeared to me to have a better action during convalescence, in a few daily doses of from one to three grains, than during the height of the fever. There are cases, however, in which a larger dose has the effect, often experienced, of prolonging the action of any of the modern phenol preparations. A very large dose, say of thirty grains, would combat supra-orbital neuralgia, where smaller doses or other sedatives had failed. In a series of influenza cases, from ten to twenty grains of quinine were taken in the forenoon, a single dose of phenacetin, of from twelve to twenty-five grains, in the afternoon, and a Dover

powder at night, with pleasant effect. There is not one of the modern remedies but whose virtues have been tried and extolled. Acetanilide ought to be preferred among the poor because of its low price; antipyrin mainly where great solubility is required for the purpose of its administration in rectal and subcutaneous injections. Phenacetin is preferable to either, because of its less uncomfortable effect on brain, heart, and skin.

Gastric irritation behaves best under resorcin; gastro-intestinal disorders have been treated with salol, salicylates, and salicin. The first is not always well tolerated; salicylate of sodium is apt to add to intestinal irritation; salicin is perhaps the most effective and least irritant of all. The dose for an adult is from five to fifteen grains every two or four hours; those of T. I. Maclagan (*Lancet*, January 11th) are larger. He gives, for three or six hours, twenty or forty grains hourly, after that time every two hours, and later on in longer intervals. By this treatment he claims to have obtained the beginning of convalescence within from twelve to twenty-four hours. I find it difficult, however, to believe that anything like abortion can take place, in response to internal medication, in a disease the invasion of which is sudden, incubation exceedingly brief, and defervescence to be expected after two or four days.

Infants and children require all of these remedies in proportionately large doses.

Mr. President, I am aware that I shall be followed by those who have ample experience and vast knowledge of the subject under discussion, and that my remarks must be brief. I shall, therefore, add a few fragments only to say that, while the treatment of the acute attack may permit of laxity and leeway, that of some of the complications and sequelæ taxes the heart and brain of the physician to the utmost. To this very hour I am under the ban of the impression made by men, women, and children perishing by the dozen of pneumonia, broncho-pneumonia, and pleuro-broncho-pneumonia. I need not tell my

audience that never have we seen so many cases of pulmonary disease of a peculiar type and fatal character in a few weeks. The subject will be discussed by one of the gentlemen who are to speak to-night; thus I shall refrain from everything in this connection, with the exception of a few remarks on the therapeutics of these severe cases. I venture to make them, though perhaps they be but a repetition of what every one here is fully acquainted with.

The great danger is prostration, exhaustion, heart failure, particularly in very old age, after previous illness, in phthisis, chronic pneumonia, emphysema, kyphosis, cachexia, alcoholism, nephritis. Wherever vomiting is severe, feeding by mouth is an impossibility, and absolute abstinence adds to the general exhaustion. Thus rectal injections must take the place of stomach feeding. May it not be forgotten by my younger colleagues that, besides alcohol much diluted and peptones in solution, nothing is better absorbed by the rectum and colon (the latter to be reached in the adult by a long tube attached to the nozzle of the fountain syringe) than egg stirred in fairly large quantities of fluid; that the quantity to be injected may be as large as from a half-pint to a pint, if it be made to reach the colon; and that starch water is preferable to water, inasmuch as starch is transformed into dextrin in the larger intestines.

When there is excessive perspiration, fair quantities of fluid must be injected at regular intervals to repair the loss.

Severe vomiting is best alleviated by morphia, internally if possible, otherwise subcutaneously, or to the oral cavity in such a manner that a few drops of Magendie's solution are poured into the hand or a spoon, undiluted, and sucked down along the mucous membrane, which absorbs it very readily.

Very high temperature prevents absorption from the stomach. The readiest method to reduce it, particularly when the stomach rejects remedies, is the subcutaneous

use of antipyretics, whenever the rectum is either wanted for feeding purposes or is not in a fit condition to retain and absorb. Antipyrin may be so used in five parts of water, the carbamide of quinia in the same proportion.

Suffocation with cyanosis has been relieved, and time gained, by the inhalation of oxygen, a few minutes in succession, every ten or fifteen minutes. The current of the gas must be introduced into the larger nostril, for the cyanotic patient, struggling for breath, does not use his mouth intelligently for breathing purposes. If the stomach be in good condition, Siberian musk, in doses of from five to ten grains, every half or whole hour, or two hours, will act satisfactorily.

The worst cases of heart failure give your internal medicines no chance whatsoever. Subcutaneous cardiac stimulation is the only method promising any effect. Alcohol, pure or diluted, will act as an adjuvant only; indeed, it is not advisable to rely on one remedy where every lost minute may tell against the victim. Sulphate of sparteine dissolves readily in water, and may be injected in doses of from half a grain to a grain every half-hour or hour, according to necessity; as it is readily eliminated, no cumulative effect need be feared. Salicylate of caffeine and sodium (the benzoate is not quite so soluble) dissolves readily in the double quantity of water, so that a syringe holding half a drachm contains about a dozen grains of the double salt, equal to about six grains of caffeine. Camphor dissolved in five parts of oil of sweet almonds, when slowly injected, is hardly ever painful, and much more pleasant than the solution in alcohol or ether. Convallamarin, in doses of from one-hundredth to a twentieth of a grain, has also yielded satisfactory results in some instances. One-twentieth of a grain of strychnia adds to the effect of those enumerated above; the tact of the practitioner and close observation of the heart beats, pulse, and general condition of the patient, who under such distressing and threatening circumstances must not be left to the care of a nurse, must decide which, and how many,

of the remedies advised are to be employed, and how often, and at what intervals, they must be repeated. I know that not a few, who seemed hopeless, have been saved by strict personal attention.

CLINICAL NOTES ON THE COMPLICATIONS AND SEQUELÆ OF INFLUENZA.*

By F. P. KINNICUTT, M.D.,

NEW YORK.

THE marked predominance of symptoms which must be referred to a more or less profound disturbance of the nervous system, will be generally admitted as stamping the epidemic of influenza which has prevailed so extensively in our midst during the past several weeks. Such symptoms have characterized not only the onset and the febrile stage of the disease, but have been prominent in its complications and sequelæ.

Not a few cases which have fallen under my observation, and which must be regarded as true instances of infection, have been limited to such manifestations. They have consisted often of a slight feeling of dizziness at the onset, headache, occasionally nausea, more or less severe pain in the back and limbs, accompanied with a feeling of prostration and mental depression; the attack running its course in twenty-four or forty-eight hours, and with the preservation of a normal temperature throughout. The aggravated character of several of the above symptoms in more acute attacks need not be referred to. The rarer nervous phenomena which have been observed, both during the attack and as sequelæ, are of great interest. It is desirable to accumulate individual experience on this point. I shall therefore venture to record my personal observations.

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* Remarks made before The New York Academy of Medicine, February 6th, 1890, in a discussion upon Epidemic Influenza.

Obstinate and acute neuralgias have occurred as sequelæ in many of my cases, of the trigeminal and sciatic nerves most frequently. I have seen two cases of peripheral neuritis of moderate severity, but not accompanied by atrophic symptoms. The mental depression which has been so prominent and common a manifestation need not be referred to further than to call attention to the suicidal impulse that has occasionally been associated with it. I have observed one case in which this was present. More or less active delirium during the attack, with very moderate elevation of temperature, has been present in several cases. A nurse in St. Luke's Hospital suffered from vivid hallucinations of sight for several days following the febrile stage. Obstinate and painful cramps in the calf muscles, manifesting themselves during convalescence, I have seen in several cases. An absence of the knee jerk, mentioned by Da Costa in his recent paper, without reference to its association or not with a peripheral neuritis, has not come under my notice.

In one patient, a male adult, certain painful symptoms, affecting the legs only, were believed to be due to a vasomotor paresis. In two patients under my observation, a child five years of age and a woman of forty-eight, characteristic attacks of herpes zoster were developed during apparent convalescence.

In a single family I have had the opportunity of seeing, with Dr. Beattie, of this city, the following group of cases: Three children were attacked almost simultaneously with symptoms of influenza, fever, in the older children (four and six years) headache and moderate tracheal catarrh. In all, the onset was accompanied by a general convulsion. In one case, only a single convulsion occurred; broncho-pneumonia was developed later, from which the patient has not yet recovered. In a second child, the influenzal attack was of slight intensity and short duration, but general convulsions occurred from time to time during the following week and in the absence of all other symptoms. The third patient was an infant of

eighteen months. Convulsions continued to occur until death, one week from the beginning of its illness, with an absence of other symptoms and physical signs than those of a moderate bronchitis. Such varied disturbances of the nervous system occur in few infectious diseases, and yet I believe they must justly be regarded as having their origin in a specific infection.

In conclusion, I desire to refer to certain features of the pneumonia which has prevailed so extensively during the epidemic.

The two forms, broncho and primary lobar pneumonia, have occurred with nearly equal frequency in my experience, including both hospital and private cases. I have been impressed with the unusual severity of the former form in adults, and with the unusual amount of capillary hæmorrhage, as indicated by the presence of a very considerable quantity of bright-red blood in the sputa, and with the frequency with which the latter form has involved both lungs. The diagnosis of primary lobar pneumonia was based upon the clinical course and physical signs of the disease, confirmed in a number of cases by autopsy (entire lobes in both lungs being in a state of uniform hepatization). Many cases which were undoubtedly examples of broncho-pneumonia have presented the physical signs of an unusually extensive consolidation for this form.

In a case of pneumonia terminating fatally in St. Luke's Hospital, the autopsy disclosed a perfectly uniform and complete hepatization (red) of an entire lobe of one lung (the typical lesion of primary lobar pneumonia), and small scattered patches of collapsed pulmonary tissue and pneumonic consolidation (equally typical lesion of broncho-pneumonia) in the other. In other words, a primary lobar pneumonia apparently co-existed with a broncho-pneumonia in this case. Not a few cases of *apparent* pneumonia which have rapidly terminated fatally, I am inclined to believe were really examples of simple massive congestion, associated with feeble heart, either previously diseased or

affected by the profound nervous depression accompanying the influenzal attack.

The above observations point to the exceptional severity of the pulmonary affections accompanying the present epidemic of influenza.

Through the kindness of Drs. Cutler and Bishop, of the house staff of St. Luke's Hospital, I am able to append the following summary of all cases of pneumonia treated in the wards during the epidemic: Lobar pneumonia, fifteen cases, seven deaths—both lungs involved in four cases. Broncho-pneumonia, nine cases, three deaths.

THE DERMATOSES OF INFLUENZA.*

By RAMON GUITERAS, M.D.,

NEW YORK.

I HAVE tried, in considering the subject before us this evening, to dwell especially upon some one branch of symptoms or complications, and for this reason have chosen some of the various cutaneous efflorescences which have occurred as concomitant with, or the result of, the disease in question, and to mention incidentally some accounts of complications and statistics which I have derived from interviews with various other physicians. In considering these various eruptions, the one which strikes us as by far the most important, especially from a diagnostic point of view, is the erythema which occurs in certain cases, and which so particularly marks them that in considering this variety I shall classify it separately under the name of influenza erythematosa. This form of influenza so closely resembles scarlatina that a very careful diagnosis is often required to differentiate them; I will therefore consider some of those cases clinically, as in so doing we may get a better idea of them.

CASE I.—Clara S——, aged nineteen, was reported to the Board of Health as a case of scarlatina, and the Board was notified that the family desired her removal to the hospital. Dr. Dillingham, the diagnostician of the Board, was accordingly sent to investigate. Patient had been exposed to scarlatina some time before this; she complained of severe frontal headache, had frequent vomiting and great prostration. Examination revealed girl suffering

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* Read at the meeting of The New York Academy of Medicine, February 6th, 1890, in a discussion upon Epidemic Influenza.

from an acute febrile attack, temperature 103° F., pulse 100, markedly prostrated, the conjunctivæ congested, and an erythematous blush present over face, chest, and upper extremities. On examining the tongue it was seen to be coated and enlarged, and reddened papillæ were seen on sides near tip. Considerable congestion of the fauces was also present. Dr. Dillingham said that, although he considered it a very suspicious case, it was nevertheless undoubtedly influenza, and advised isolation. On the following day the eruption had disappeared.

CASE II.—A similar one in a man, aged thirty-six, who had been exposed to scarlatina. Isolation ordered. Eruption disappeared on following day.

CASE III.—Child aged six, temperature 104° F., marked delirium, followed in a few hours by erythematous blush and symptoms above mentioned.

CASE IV.—Maid in private family, aged twenty-two. I had treated a case of scarlatina of a very mild type in the same family the week previous, in which the temperature did not go above 102° F. January 6th, was first called to see patient. She had been vomiting and suffering great gastric distress, severe frontal headache, marked prostration, etc. Erythema of face, chest, and upper extremities. Temperature 103° F., pulse 108. Angina, furred tongue, and enlarged papillæ on sides near tip. Eruption faded in twelve hours.

Dr. Lynde, of the New York Foundling Asylum, reported six similar cases, in all of which the eruption disappeared in twelve hours. These cases all occurred in adult females, ranging in age from twenty to thirty years. Several other physicians in this city have noticed similar eruptions.

Dr. Simpson, of Plainfield, N. J., reported such a form of the epidemic as being quite prevalent there among children. According to the cases noticed by observers in this city, however, it would seem that it is more prevalent among adults, and that, too, in adult females. The resemblance between this erythema and scarlatina is at first glance a striking one, but a careful examination will reveal many points of difference, *e.g.:*

Erythema.	Scarlatina.
Temperature not as high.	
Pulse not as rapid.	
Eruption only over face, chest, and upper extremities, not punctate, but a general pinkish blush.	Eruption over face, chest, and both extremities, begins in a fine, punctate manner, tends to coalesce, and is of a bright scarlet hue.
Angina not as marked.	
Tongue has thick white coating, with enlarged papillæ on sides near tip.	Tongue not as thickly coated, but more markedly strawberry.
Skin moist.	Skin hot and dry.

Herpes labialis has been observed in several cases, occurring principally in the catarrhal form of the disease, appearing generally on the second day and lasting for three or four days.

Miliaria has also been observed occasionally during the epidemic. In one case, where it was quite marked over neck and chest, patient was suffering from the gastric form of the disease. It generally occurs in the papular form, and is only of a few hours' duration.

A peculiar papular eruption, occurring discretely over neck, arms, and chest, has been observed by Dr. Byron in several cases of influenza among the Italians seen at the New York Dispensary. The papule is quite large, with an irregular base, and of a bright red color, closely resembling an erythema multiforme. It appeared generally on the second day, and disappeared in two or three days.

Urticaria has also been reported as occurring in some cases of the gastric type of the disease, coming on at onset and lasting for about twenty-four hours.

Various other eruptions have also been mentioned as occurring during the epidemic, but not in sufficient numbers to warrant their mention in connection with the disease. For instance, one observer mentioned a case in which the rheumatic pains were intense, and which was accompanied by a furunculosis scattered over the entire body.

Again, a woman, aged forty, who had been suffering from influenza for some three weeks or more, accompanied by severe rheumatic pains, was suddenly attacked by

an eruption, eczematous in character, scattered in patches over body and extremities, which is still present.

These two latter cases I simply mention as occurring coincidently during the epidemic, and attach no further importance to them.

After reviewing the various other groups of eruptions above mentioned, most of which are erythematous in character, it seems to be an open question whether they are dependent upon the disease directly or the gastric complications accompanying it.

Acute Hyperæmia of Tongue.—Dr. Burke reported seven cases of sore tongue, in which cases the tongue was enlarged, reddened, hard, and tender, so that the pressure of the teeth against it was quite painful. There was also a feeling that the tongue was too large for the mouth, and the patients went around with theirs protruding somewhat and resting upon their lower teeth. The papillæ were enlarged and very sensitive, especially the group situated on either side and near the middle. This came on generally on the second day and lasted about a week. This complication did not seem to occur with any particular type of the disease, although some angina existed in most of the cases. The doctor had also suffered from a similar attack. I was sorry not to have had sufficient time to make further inquiries into this particularly interesting feature of the disease, but Dr. Burke was one of the last men visited by me. I remember, however, mention having been made of a similar case by another observer, but at the time attached no further importance to it.

Statistics from Deaths Reported.—It might be interesting, perhaps, to read a few statistics tabulated from a report which I am preparing for the Board of Health regarding the various deaths reported as due to influenza. The newspaper reports were in most cases grossly exaggerated, and simply served as sensational food for the masses.

The number of deaths reported to the Health Board as due to influenza during the epidemic was one hundred and twenty-one.

Of these I have investigated eighty-six through their attending physicians, and find that the causes are somewhat as follows:

Lobar pneumonia	33
Lobular "	21
Bronchitis	13
Meningitis	4
Other diseases	15
Total	86

Of the remaining thirty-five deaths, I find from the death certificates that they resulted from the following diseases:

Pneumonia	19
Bronchitis	8
Other diseases	8
Total	35

Of the thirty-three deaths reported as due to *lobar pneumonia*, I find that twenty-eight of them, or eighty per cent, were in men, that only four of them were over fifty years of age, and only one was under twenty, he a child of but seven days. The majority were between the ages of thirty and forty-five.

The duration of the disease was from two to five days in most of the cases, the quickest fatal case being of one day's duration, and the slowest of eleven days.

In Lobular Pneumonia, on the other hand, fifteen of the twenty-one deaths were in women, and of the six occurring in males, four were over fifty years of age. The general age also differed very much, as they were in most cases younger or older than those of the lobar variety. The duration also differed, the disease generally not covering as long a period.

Bronchitis also seemed to be more fatal in women, as in thirteen cases nine were in women. It was also especially fatal in the aged, most of the deaths occurring in women over fifty years of age.

Meningitis.—Four deaths were due to meningitis. They were all in males. Three of them were between the ages

of twenty-four and fifty, the other a child of three years. The average duration was five days. The remaining fifteen deaths were as follows:

Valvular disease of the heart	2
Phthisis	2
Dysentery	2
Nephritis	2
Sarcoma	1
Eclampsia	1
Peritonitis	1
Pleuritis	1
Influenza	2

Thus we see that there were only two deaths due directly to influenza, and of these one was in an Italian, sixty-one years of age, in a weakened and debilitated condition, the other in a baby of eight days. In these fifteen cases above mentioned the influenza was the determining cause, and frequently made rapid work, some of them succumbing in a few hours, apparently from the shock of the onset.

In reviewing the history of the manner of dying in these cases, one cannot but remark how many of them were due to "heart failure" and a peculiar "choking-up,' as spoken of by the various physicians in relation to deaths in broncho-pneumonia and bronchitis, causing death by asphyxiation.

This would seem to point to the fact that the prostration occurring with the influenza is the most dangerous factor in the disease, and that it seems to exert an especially depressing influence on the action of the heart and lungs, as was shown by the tendency to "heart failure" and the diminished power of expectoration.

THE COMPLICATIONS AND SEQUELÆ OF INFLUENZA.*

By WM. H. DRAPER, M.D.,

NEW YORK.

I SHALL devote the few remarks which I have to make in this discussion to a brief consideration of the complications and sequelæ of the epidemic influenza. These complications and sequelæ may be described for the most part as exaggerated expressions, or a more or less protracted persistence, of the ordinary symptoms of the disease. They are, therefore, like the symptoms, divisible into two principal varieties: First, those which are included in the sensory-motor derangements which result from the effect of the materies morbi, whatever it may be, upon the cerebro-spinal system; and, secondly, those which illustrate the manifold and severe lesions which the disease determines in the mucous membranes and parenchymatous tissues. Sometimes the disease manifests itself mainly by its cerebro-spinal symptoms, and sometimes the catarrhal lesions constitute its prominent feature; but commonly it presents a combination of more or less profound derangement of nervous functions with varied forms of catarrhal and parenchymatous affections. In many respects epidemic influenza presents striking analogies to the other infectious fevers, and notably, perhaps, as has been repeatedly observed, to epidemic cerebro-spinal meningitis. In its sudden and often violent mode of onset, in the variety and occasional intensity of its ner-

*Remarks made before The New York Academy of Medicine, February 6th, 1890, in a discussion upon Epidemic Influenza.

vous symptoms, in its irregular febrile movement, in its manifold lesions, and in its marked tendency to relapse, it certainly often reminds us of this form of epidemic meningitis. All the symptoms suggest irritation of the cerebro-spinal axis, more or less severe, and it seems probable that the lesions of epidemic influenza, like those of epidemic meningitis, are determined by vaso-motor derangement.

I have said that the complications of epidemic influenza are, as a rule, only exaggerated expressions of its ordinary symptoms. To the chill, headache, and more or less severe malaise which commonly characterize the attack, there may be added delirium, and in children convulsions; cases have been reported which, in their onset, have been mistaken for meningitis. In some instances the nervous shock manifests itself by giddiness and faintness, with an abrupt and inexplicable failure of mental and muscular power which may surprise the victims on the street or at their occupations, and compel instant capitulation to this subtle and insidious disease. The mental hebetude which, in varying degrees, is a common phenomenon, may even assume the features of melancholia, and cases of suicide have been recorded in this as in previous epidemics. The muscular paresis is often so extreme as to render patients quite helpless. The sensory disturbances are at first more or less generalized in the form of cutaneous hyperæsthesia, muscular pains, and soreness. Definite neuralgias in the distribution of important nerve trunks, though occasionally observed at the outset of the disease, are, I think, more often consecutive phenomena. These neuralgias affect especially the cervico-occipital, the trigeminal, the intercostal, the lumbo-abdominal, and the sciatic distributions. They often are intermittent, like the neuralgias of malarial origin, but without any regular periodicity. They are sometimes accompanied by exquisite tenderness along the course of the affected nerve, and are much aggravated by movement—two features which seem to indicate that the lesion which determines them is a perineu-

ritis. A few cases have fallen under my notice in which the symptoms of an interstitial, and possibly even of a parenchymatous, neuritis were present. In one case of this description, which occurred during convalescence from catarrhal pneumonia, the musculo-spiral and median nerves of the left arm were involved. Paralysis of the flexors and extensors of the forearm followed, with considerable atrophy. A paretic condition of the limbs, following brachial and sciatic neuralgias, without ostensible atrophy, has been a common observation. I have not seen any trophic disturbances in the skin, excepting in connection with the trigeminal neuralgias; conjunctival congestion and herpetic eruptions about the mouth have been frequently observed. As a rule, I think the neuralgias occurring as sequelæ of influenza have been confined to the distribution of single nerve trunks, to a branch of the trigeminal, generally the supra-orbital, of the brachial or sciatic plexus, or to an intercostal nerve on one side, though I have seen a number of cases of double sciatica, and one of multiple neuritis, which I believe to have been of influenzal origin.

A conspicuous feature of the nervous phenomena of influenza has been their protracted duration. This has been manifested frequently in the persistence of the neuralgias, and especially in a continuance for some weeks of muscular weakness, fatigue on slight exertion, mental depression, in a feeble and rapid pulse, and generally in a slow and halting recovery.

The complications of the catarrhal form have presented themselves most frequently in the respiratory tract. Cases of acute pharyngitis with intense congestion of the mucous membrane, extending sometimes into the Eustachian tube and causing otitis media, have not been uncommon. Laryngitis and tracheitis, accompanied by a paroxysmal, pertussis-like cough, have also been observed, complicated in such a way with the nervous symptoms of influenza as to leave no doubt of their specific origin. But the most serious and important complications of the

catarrhal form of the disease have been bronchitis, broncho-pneumonia, pleurisy, and pleuro-pneumonia. The peculiar feature of the bronchitis, in my experience, is that it has often been confined to one lung or to a portion of one lung. It has not usually been accompanied by much expectoration, but rather by feeble respiration, with only occasional moist or dry râles on forced inspiration. The development of a lobular pneumonia from this condition has generally been slow, and the signs of consolidation often difficult to determine after the subjective signs have suggested its existence. A localized pleurisy is sometimes superadded to this condition.

In the cases of lobar pneumonia the onset of the disease has, I think, been as characteristic in its suddenness, and the progress of the consolidation has been as rapid, as is ordinarily observed in croupous pneumonia. It has seemed to me that the pleuritic element in the severe cases has been unusually pronounced, the pain intense and widely distributed, and the tenderness of the costal and abdominal walls extreme. I have also noticed that even in cases of large pulmonary lesion the vital signs did not present the characteristic features which usually prevail in ordinary lobar pneumonia. The range of the temperature has not, as a rule, been so high, and the pulse-respiration ratio has been more irregular. Defervescence has, in my observation, seemed to occur earlier, and the resolution of the lesion to be slower. The slow resolution has been due in some cases to the pleuritic complication, which, as I have before remarked, has been unusually severe.

The complicating lesions in the gastro-intestinal canal have not, in my experience, been common, but they have occurred with sufficient frequency in this and former epidemics of influenza to justify the statement that the disease may spend its force in this direction, and without the appearance of the usual pulmonary symptoms. In some instances the gastro-intestinal derangement has been so severe as to suggest the existence of typhoid fever, espe-

cially in the exceptional cases where the fever has continued for a week or longer, in connection with abdominal pain and tenderness, with gurgling in the ileo-cæcal region. In two such cases which I have seen the stools contained blood.

I have not observed any complications affecting the genito-urinary tract. Cases of cystitis and of acute uterine catarrh have been described, accompanied by the general constitutional disturbances which characterize the grip; and though one is too prone, perhaps, during the prevalence of such a disease, to multiply its manifestations, it is not improbable that the morbid influence complicates, if it does not determine, a great variety of structural derangements.

Many observers in this and in former epidemics have described various forms of erythema complicating influenza. I have several times observed herpetic lesions, especially about the head and face, with œdema of the lids, associated with neuralgia, but I have probably overlooked the scarlatiniform eruption which has been reported in many cases. The occurrence of cutaneous, and even of arthritic, lesions in a disease which often exhibits such remarkable cerebral and peripheral nerve irritation, is certainly to be expected.

In conclusion, I have only to note the remarkable frequency of relapses in this disease, and also the fact, especially noted in hospital patients, that the relapse is more severe and accompanied by more serious lesions than the original attack. The tendency to relapse, while frequently the result of imprudence, has often been observed in those who have been carefully guarded against exertion or exposure.

From this brief and imperfect sketch of the complications and sequelæ of epidemic influenza, it seems evident that the determining cause of the disease, whatever it may be, is one which spends its force, primarily and directly, upon the nervous system. Its most serious results have, as a rule, been observed in persons of advanced years, and

in those who were enfeebled by previous disease or intemperate habits.

The relative mortality of the malady has, to be sure, not been large, but when we consider, apart from the mortality it has caused, the vast sum of human energy that it has destroyed, and the check that it has everywhere put upon the wheels of human industry, its effects have indeed been appalling.

THE THERAPEUTICS OF CARDIAC DISEASE.*

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When we speak of the therapeutics of cardiac disease, what is it that we mean? To what are our remedies addressed? What are the conditions that pathology has unfolded that are amenable to treatment?

We are concerned with the heart both in its anatomical as well as in its physiological aspect. Next to its position and size comes the consideration of the heart muscle; of this muscle as a vast mass making up the ventricular and auricular walls, acting through columnæ carneæ and papillæ upon tendons which control the action of valves; of these valves themselves; of the endothelial layer, the nerve distribution of the organ, its own blood vessels, and, finally, of its relation to the circulation and circulatory fluid and some of the more important viscera, as the lungs and kidneys.

Careful examination reveals to us what changes have taken place, and clearly and sharply must we decide which are the pathological conditions to which remedial agents shall be applied; which factors first demand, and for how long, our attention; when shall consideration be given to others; and, finally, when shall we cease in our attempt to influence the heart itself, having attained the limit of scientific therapeusis.

While it is not within the province of this paper to wander strictly from the realm of therapeutics, still I think it not amiss to group together, so as to have before

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* Read before The New York Academy of Medicine, February 20th, 1890.

us, what are now considered the important lesions of cardiac disease, and for some of which treatment may be attempted.

At the outset I would urge attention to the probable congenital condition of the heart and arteries. Abnormally small hearts, associated with imperfectly developed aorta and blood vessels, are by no means a rare congenital condition. Similarly other and less frequently occurring congenital abnormalities are met with. In most instances, however, we meet with acquired disease. Thus we are called upon to treat either disorders of the heart muscle, or else these associated with valvular lesion or lesions of the orifices; or with atheromatous and sclerosed arteries of the heart or of the aorta, or of the entire arterial system. Still further, with the latter combination in which the arterial involvement is the result of extraneous local causes, as tumors pressing upon them, or constitutional degenerative changes proceeding from other viscera, as disease of the lungs or renal system. Finally, we meet the category in which the nervous control or distribution of the heart is at fault, in which the heart muscle may remain normal, or be itself ultimately secondarily changed, hypertrophy, dilatation, and degeneration supervening sooner or later. Such are the cases of tachycardia, abnormally slow heart, intermittent and arrested heart beat, and some of the obscure cases known as angina pectoris.

The condition of the blood has an important bearing upon the condition and progress of many cases of chronic heart disease, and the consideration of the two conditions —plethora and hydræmia—in connection with a given case, frequently furnishes important data for treatment. Bollinger has well established the fact that a condition of plethora alone, maintained for some time (as is evidenced in persons, as beer drinkers, who overeat and overdrink, and exercise comparatively little, who in consequence have a pulse of high tension), leads to hyperæmia and hypertrophy, and ultimately to dilatation and insufficiency of the cardiac muscle.

Similarly hydræmia, with its increased quantity and its involved impoverished albuminoid condition of the blood, results in degenerative changes similar to those ultimately produced by plethora.

To make a brief *résumé*, then, of the various cardiac conditions we are called upon to treat, would give us:

a. Hypertrophy, concentric or eccentric, alone or in combination with dilatation.

Dilatation alone or combined with hypertrophy.

b. Hypertrophy and dilatation, or combined; together
1, with mitral disease (stenosis or insufficiency);
2, with aortic disease (stenosis or insufficiency);
3, with mitral and aortic disease in their various combinations;
4, with disease of the pulmonary artery, or else this with
a, mitral disease (stenosis and insufficiency);
b, aortic disease (stenosis and insufficiency).

If we group our cases upon an etiological basis, we have hypertrophy and dilatation, either or both, or combined with valvular disease, the result of

a, congenital causes;
b, work or excess;
c, fright and hysteria, and other nervous causes;
d, indigestion, malassimilation, etc.;
e, inflammation (causes producing endo- and pericarditis, myocarditis, and interstitial myocarditis);
f, degeneration;
1, fatty infiltration;
2, fatty, granular, waxy degeneration;
g, either inflammation or degeneration, the result of infectious diseases;
h, or sclerosed coronary arteries (thrombosis and embolism, chronic fibroid degeneration of heart muscle, or both of these combined);
i, or obstructed circulation, acting secondarily through changed blood state (tumors and aneurisms pressing

or interfering, pulmonary diseases, as emphysema, etc., renal disease).

Contracting our enumeration of all cases still more, we have hypertrophy and dilatation, either or both, or combined with valvular disease and associated with—*a*, plethora; or, *b*, hydræmia.

It is the natural tendency of a viscus to seek to restore itself to a normal state or to adapt itself to new conditions and surroundings. Nowhere is this better illustrated than in the heart muscle, which, by a system of compensatory hypertrophy, seeks to resume its normal vigor and functions.

As long as this holds good, irrespective of the nature of the lesion, little or no treatment is required at our hands. In all cases it is the insufficiency of the cardiac muscle and the resulting lack of presentation, whether evidenced by dilatation of the heart wall, by intermittent or irregular action, or by weakened cardiac impulse, which furnish the keynote for treatment. Similarly, whatever we may be enabled to do in advance to prevent this latter change is a proper object of therapeutic endeavor.

In the attempt to tabulate all our means of combating cardiac disease, their arrangement under the headings of hygienic, dietetic, remedial, and mechanical agents is sufficiently broad to include all.

Some act temporarily, some permanently, some locally, others constitutionally. While a few act directly upon the heart muscle, most act indirectly by changing blood states or restoring altered nervous tone. Finally, it must be confessed that some act in a manner whose *rationale* is not entirely clear or satisfactory, but whose results force us to adopt the measure; while, conversely, we must deplore our failure to influence some conditions of the heart, despite all our methods and multiple resources. In speaking of the treatment of cardiac disease, I need not refer to that general treatment which any case, whether of cardiac origin or not, may require, for the more perfect is the general health the better does each individual organ

perform its function. Here, as in other diseases, specific treatment directed to the heart itself may fail, when general supporting treatment will accomplish the result.

Under remedial agents we have, as most important, digitalis, strophanthus, convallaria, sparteine, and adonidin; veratrum and aconite; ergotin and strychnia; the general tonic remedies, cod-liver oil, iron, cinchona, and the like; bromides, belladonna, hyoscyamus and its congeners; iodides; galvanization.

As temporary agents only, we have alcohol, caffeine, and theine; ether, chloroform, ammonia, camphor, and musk; nitroglycerin, nitrite of amyl, opium, counter-irritation, faradization, zinc, arsenic, phosphorus, barium salts, venesection.

The dietetic treatment includes the regulation of the relative solid and liquid foods, and the absolute control, as to the character and quantity, of both, depending upon indications of the cardiac condition and the blood-state.

The mechanical includes massage and exercise, by walking, by climbing, by medical gymnastics, active or passive movements, or else a combination of these, and, finally,

Medicinal Baths.—These latter may likewise be aided by some form of exercise or massage.

Hygienic Remedies.—The limit of time for the evening forbids any very detailed consideration. The effect of unhealthful trades and atmospheric surroundings must be guarded against, as is forcibly illustrated in the tobacco manufacture, and in others to a less degree. Tobacco and alcohol both give rise to cardiac disturbance, and may lead directly to cardiac overaction and hypertrophy.

Dietary Treatment.—We are so long accustomed to associate certain diatheses as tending to predispose to cardiac disease that it is a matter for surprise that the dietetic treatment has been until recently, although spoken of in books to a slight extent, carried out in practice even less.

It is not uncommon* to meet with cases of cardiac irregularity and more or less dilatation in which the sole cause seems to have been an insufficient and improper diet, associated with bad hygiene and overwork.

The quantity and quality of the food is, therefore, the first consideration. This will give us a circulating fluid which leaves the heart unembarrassed, and does not tend to fatty deposit, to the gouty and other diatheses.

The indiscriminate use of water during meals embarrasses digestion, and may, in cases of fatty heart, after a hearty meal, if exercise be attempted immediately, give rise to heart failure.

Oertel's indications for the dietetic treatment are clear and explicit, and are briefly summarized in the following:

1. In cases of chronic heart disease, in emaciated or slightly built persons, associated with anæmia and hydræmia and with insufficiency of cardiac muscle:

Increase the quantity of albuminoid foods, the object being to increase the muscle element, not only up to the normal state, but even to a condition of hypertrophy and hyperplasia.

Rest and fat formation on the one hand, exercise and blood and muscle formation on the other, are the things to be sought after.

2. *a.* In cases of abnormal adipose tissue, with plethora and commencing insufficiency of cardiac muscle:
   1. Increase the amount of albuminoid food.
   2. Diminish the amount of fat-making substances.
   3. No change in liquids other than is physiological.

   b. In cases of abnormal adipose tissue, with hydræmia and insufficient cardiac muscle:
   1. Increase the amount of albuminoid food.
   2. Diminish the fat-making substances.
   3. Diminish the quantity of fluids.

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* Both in the Polyclinic and at the Mount Sinai Hospital quite a number of Russian immigrants, after two or three years of labor of twelve to sixteen and eighteen hours daily, fed upon bad diet and in poor surroundings, have been met with suffering in this way.

c. In cases of adipose deposit in old persons, with hydræmia and commencing insufficiency of cardiac muscle:

1. Increase the albuminoids.
2. At the outset continue the fats and carbohydrates, yes, even increase them for a time, in order to prevent the too rapid diminution of fats when this begins to take place—an occurrence characteristic of old persons when their dietary is in any way interfered with.
3. Diminish and regulate the fluids.

As regards the relative amount of foods indicated for the adipose cases, Oertel gives:

	Albuminoids. Gm.	Fat. Gm.	Carbohyd. Gm.	Fat. equiv. Gm.
Maximum	170	43	114	173
Minimum	156	22	71	127

Ebstein gives, in his treatment of the fatty diathesis:

	Albuminoids. Gm.	Fat. Gm.	Carbohyd. Gm.	Fat. equiv. Gm.
Daily average	102	85	47	154

In the cases of adipose deposit more particularly associated with hydræmia, and in older persons, food must be given in small doses and often, and fluids must not be given with solid food.

By abstaining from fluids fat absorption is promoted.

Finally, when fat disappears new formation of blood and muscular tissue is called for in the body, and the fat is replaced by muscular tissue.

In these cases exercise is used, not to increase the amount of albuminoid deposit so much as to assist in the elimination of fat. This overfeeding continues only up to a certain limit with marked increase of blood and muscle formation; when, however, fat is deposited in large amounts in the body, the quantity of blood diminishes and anæmia supervenes.

That these rules cannot fix absolute limits will, I think, be readily appreciated; each case must be treated with

some regard to the idiosyncrasies and peculiarities of the individual or the heart itself. Of course, it is apparent to all that the promise of success from the dietetic method depends upon so many factors that disappointment is not infrequent. It is so difficult to retain the unwavering support of the patient that herein is the element of greatest uncertainty.

Remedial Agents.—It is worthy of note, in using the cardiac stimulants, that the combination of several of them will often give better results than the use of either of them alone. Still further, by the combination unpleasant effects are often avoided.

Thus strophanthus and convallaria give better results combined. My own experience has led me to prefer tincture of strophanthus to the alkaloid strophanthine.

When digitalis is combined with strophanthus, using the former in small doses, we obtain all the good results of each drug without the cumulative effect of digitalis.

Calomel at times increases the efficacy of digitalis, whether by relieving the congestion of the intestinal tract and improving the hepatic circulation is a matter of conjecture. In using temporary cardiac stimulants, the previous habitual use of tea or coffee or alcohol gives us an indication or contra-indication for the use of theine or caffeine salts, or the natural beverages. The habitual use of large quantities of coffee by the Germans has led Fürbringer to adopt the use of tea or theine in his ward as a cardiac stimulant, and with markedly good results, whereas caffeine had previously proven abortive. Musk, so long recommended, is looked upon with scepticism, and unjustly so; too small doses account for its inefficacy. Recent experience has taught me that by addressing myself to the drugs which act upon unstriped muscular fibre, I have succeeded in decidedly improving the tone of the heart fibre; thus, by the administration of full doses of strychnia and ergotin I have, in many cases, after previously regulating the heart's action, strengthened it beyond expectation.

In these cases nux vomica or strychnia was used in full physiological doses. I do not doubt but the combination of digitalis and quinia, long a favorite one, proves beneficial, not only through the tonic action of the cinchona, but also in part by reason of its effect upon muscular fibre.

The subject of counter-irritation deserves a passing allusion. The cautery (either the heated glass rod, the Paquelin, or galvano-cautery) applied superficially in vertical lines over the hepatic or præcordial region is a decided means of temporarily regulating the heart's action. In simple irritable heart, from reflex causes, this may be repeated with good results.

In speaking of the treatment of cardiac disease, I need not refer to that general treatment which any case, whether of cardiac origin or not, may require, for the more perfect the general health the better does each individual organ perform its function. Here, as in other diseases specific treatment directed to the heart itself may fail when general supporting treatment will succeed.

Historically we find Albertius and Valsalva firm in the belief that the condition of hypertrophy and dilatation in cases of chronic heart disease could be ameliorated.

Rest in bed, blood-letting, a limited diet, and an allowance of bouillon were the main features of their treatment. Laennec suggested similar regimen.

Later we find other authorities suggesting further changes. Blood-letting in all stages, save in the latest, continued to be recommended, and now rest in bed was replaced by consent to be up and about in an equable temperature. The diet was to be a moderate vegetable diet, with moderate use of beef and milk, and simple drinks in small quantities.

Wunderlich, besides seeking to prevent further changes in the heart by symptomatic treatment, sought to establish a definite relation between the capacity of the heart and the quantity of blood to be acted upon by it. Again blood-letting was recommended as a ready means to adapt

the heart to the task of the quantity of work to be done, otherwise his suggestions coincided with those above given.

Stokes led in the new departure, in recommending gradual and increased exertion and exercise for cases of fatty heart, suggesting moderate mountain-climbing in Switzerland and England. Only in very old persons with complications was caution in adopting this recommended; in young persons it was urged. He also suggested meat diet, and abstinence in these cases from fatty foods, soups, and vegetables. Moderate indulgence in water, with wine or whiskey, unless in advanced stages, was suggested.

Unfortunately these valuable suggestions were soon lost sight of and received no recognition.

Later, Bamberger referred to the fact that exercise had been recommended for fatty heart. It is to Oertel, however, that we are particularly indebted for the more perfect development of both the mechanical as well as dietetic plans, placing these both upon a scientific basis, and by his results inducing others to follow his example and achieve his brilliant results.

Mechanical Method.—It is a well-known fact that proper exercise of a muscle leads to its hypertrophy; even muscular fibre which has in part undergone fatty degeneration may become changed in character and the muscle assume a normal, healthy aspect. Still further, in cases where advanced degeneration has taken place, some of the healthy fibres remaining may ultimately hypertrophy and, by predominating over the degenerated fibre, restore action to a formerly partially useless muscle. And even more than this, the restored tissue, by subsequent cell growth and reproduction, furnishes new fibre, and in this manner fatty degeneration, fatty infiltration, and even interstitial degenerative changes, lose their influence, and a return to a normal, or nearly normal, function is brought about.

That these changes take place in the heart muscle is

clinically demonstrable. We have all repeatedly witnessed the more or less complete compensation in the heart, as evidenced in the disappearance of the signs and symptoms of a passive and active congestion, and in an improved heart action itself, showing that the weakened or insufficient heart muscle had been more or less restored.

Exercise increases the functional activity of the heart, causes an increased flow of blood to it, and improves its nutrition. When the heart is weakened by any cause, this effect becomes apparent upon the slightest muscular contraction; and when the entire body is put in regular motion, as may be done by regular walking, climbing, or gymnastics, the heart tends to beat more regularly and more frequently, the contractions of the muscles send the venous blood to the heart in greater quantity, the arteries become dilated and filled, the capillaries widen, a freer circulation is established, the pulse is increased in frequency, and the intra-arterial pressure is permanently increased—the increase during and immediately after exercise being very great, gradually diminishing after the period of rest, but remaining for eight or ten hours, sometimes even twelve or twenty hours. That a general influence for good results from the general assimilative molecular changes which exercise, however moderate, favors, is evidenced from the increased flow of urine and excretion of urea. Indeed, in cases of general anasarca Oertel has formulated the rule that "if the urine contain large quantities of albumin, and the difference between fluids absorbed and excreted is more than fifty per cent, and by diminishing the fluids absorbed no improvement in the percentage excreted is gained, in such cases mechanical treatment is decidedly contra-indicated." Returning to what we have just previously alluded to, it is a matter of great interest to compare the sphygmographic tracing before and after the mechanical treatment—and likewise to note the increase by the sphygmanometer of the intra-arterial pressure.

In the sphygmographic trace we note, after exercise, that the line of descent almost equals that of ascent, the recoil rise being only a fraction of that prior to the exertion. The sphygmanometer teaches us that the increase may be ordinarily as much after a fair amount of exercise as 18 to 20 mm. of Hg, this gradually diminishing to from 1 to 3 at a varying period of from eight to eighteen hours later.

Oertel claims that a well-regulated mountain climb, of very gradual ascent, is the best means of applying exercise in cases of heart disease amenable to mechanical treatment. Selecting the mountain-sides in the health resorts of Germany and Switzerland, he has carefully fixed the limits of the exertion which the individual is subjected to, by varying the length, duration, and steepness of the climb. It is not a climb at random anywhere and under any circumstances, but under one careful regulation, the amount of effort required and its results being carefully calculated in advance.

As it may not be a familiar fact to all that a diseased heart, so long as its compensation remains unimpaired, will permit of indulgence in enormous physical strain without injury in many cases, the following examples will not be without interest. A case has recently come under my observation of a lad, nineteen years old, who presented himself at the New York Polyclinic, complaining only of dyspeptic symptoms and of palpitation, who had in early youth suffered from rheumatism. Examination revealed the lesions of aortic stenosis and insufficiency, the latter a little more marked; dilatation, with well recognizable compensatory hypertrophy, was discernible, the heart beating regularly and there being no evidence of heart weakness or even of ill-health. Upon investigation I ascertained that for years the lad had been a member of a German Turnverein, and had twice or thrice weekly spent several hours hard at work in the gymnasium. In this case he had labored under the control of a competent and careful inspector, with

benefit to his heart. I well recall the case of a 'longshoreman who died at the age of sixty-nine at Roosevelt Hospital, and who, from his clearly related history, had carried a mitral lesion for fifty-two years.

If under favorable circumstances, then, a markedly diseased heart can bear so great a strain because this has to some extent been carefully regulated, it is not too much to presume that, knowing the good effects of exercise, if our cases are properly selected, some diseased hearts with insufficient muscle may, by some form of judiciously regulated exercise, be more or less improved; and more particularly when we remember that it is less through the direct effect upon the heart, but rather through the improvement obtained in the circulation and circulatory fluid, that our result is obtained.

A priori, stenosis of the mitral orifice is a contra-indication to the mechanical treatment of cardiac disease, rather than regurgitation. The means by which the mechanical method acts aid the condition and tendency of the heart muscle in the latter class of cases and retard it in the former.

In mitral stenosis the circulation is certain to be interfered with, irrespective of the condition of th cardiac muscle. In this condition the circulation feels the effect of the retardation most promptly, extending beyond the pulmonary circulation to the venous system; and the auricle compensates slowly, and only after some time. To introduce the mechanical treatment here would only increase the hindrance to the venous circulation, for the more quickly you seek to drive a fluid past an obstruction the more the obstruction becomes felt. Stenosis of the aorta, on the contrary, is soon followed by compensatory hypertrophy of the left ventricle, and here the mechanical method finds an application.

In insufficiency of the valves, more particularly of the left side of the heart, we have a class of cases in which nature soon follows the lesion by a compensation in the heart muscle itself; and because the mechanical method

acts by assisting in maintaining, in restoring, or in increasing this compensation, we have in this a mode of treatment which completes what Nature, if left to herself, only partially effects in many instances.

How long mechanical treatment should be continued in these cases is a very serious question. In addition to dietetic and other measures, these cases, even when the heart is acting well, should remain more or less constantly under observation; for, although compensation may be obtained, yet it is a matter of great uncertainty how long it can be maintained. It is therefore advisable either to continue the treatment in a moderate manner, or to replace more inconvenient methods by more convenient, or comfortable, or less severe ones, omitting the method entirely whenever a premonition of failing compensation is recognizable either in the general circulation or in the heart, particularly on the right side.

In cases in which insufficiency and stenosis are combined, the question of the utility of the mechanical method will depend upon the fact whether the stenosis or the insufficiency predominates, and, still more, whether the stenosis be that of the mitral orifice, in which the compensation of the auricular wall is at best unsatisfactory and uncertain. Still, if by first resorting to palliative means we can improve or control the latter, then this may be employed with subsequent benefit.

Finally, in cases of fatty degeneration or fatty infiltration, whether associated with plethora or hydræmia, the mechanical method, carefully applied, associated with dietetic means, is a powerful restorative of lost compensation. Frequently only after a persistent combination of the mechanical and dietetic treatment, by which means the nature of the heart muscle is more or less changed, do we find that drugs will exert any influence whatever.

By exercise and diet the increased blood supply to the heart, the dilatation of the capillaries and arteries—which involves, of course, the coronary branches as well—per-

mits increase of nourishment to the muscular tissue, by which the above result is obtained.

As to the modes in which the mechanical method may be applied, each has its votaries, who in their preference are guided largely by surroundings and ultimately by their success under these circumstances.

While Oertel's method adopts the walk or climb, or the combination of both—so that at the health resorts your eye meets the familiar sign on the hill-side, this is "the beginning" or "the end of Oertel's walk"—others find this crude and unsafe.

The Swedish movements medical gymnastics—the term given by Lingg to a systematized series of movements, active and passive—are preferred by some. I presume most of you are familiar with these movements, and will not delay you further in explaining them.

Massage may likewise be resorted to. Still further, some physicians carry out themselves, or entrust to a trained assistant, a series of movements which are resisted by the patient, such as are produced by bracing the hands against each other and the like, always avoiding any direct strain upon the heart or lungs, and increasing the severity of the exercise as habit and returning vigor upon the patient's part indicate; the aim being gradually to call into play all muscles of the body—a mild wrestling match, as it were—but without subsequent fatigue.

Baths.—It is now almost twenty years ago since the waters of Bad Nauheim, although used for many years, received systematic attention for their influence upon rheumatic, and the consequent cardiac, disorders. Jacob of Cudowa, and later A. Schott, deceased, resorted to these baths, and combined their use with medical gymnastics in some form, for the relief of heart disease. This good work has since been continued by the brother of A. Schott, Dr. Theodore Schott, as well as by Bode, Groedel, and others.

Bad Nauheim is situated in the valley of the Rhine, an hour's journey from Frankfort-on-the-Main *en route* to

Cassel. It has an elevation of four hundred metres, has a sandy soil, has a gradually inclined mountain-side at one extremity, and extends out into a wide plateau on the other sides. It contains a number of springs, more or less powerful, containing large admixture of alkaline salts, chlorides of calcium and soda, of sulphur, iron, and abundant free carbonic acid. The temperature at which the baths are given varies from 88° F. in some to 90° and 93° F. in others. The waters are used by being diluted with ordinary water at first, and the temperature and the duration of the bath and change in its character regulated after the age, condition, and daily indications of each case. The physician gives daily minute directions for the bath, and visits his patient at varying times after the bath to study and note the effects. The improvement in the blood state is exceedingly marked, and the nearer a given case of cardiac disease comes within the range of being influenced by improved blood conditions, the more certain is a good result to be expected.

Hand-in-hand with this, medical gymnastics have been made use of, and when a careful dietary has been added the opportunities of securing improvement increase.

I cannot refrain from referring to a case of an infant suffering from rheumatic purpura associated with impoverished blood state, in which all the blood vessels of the body were at fault—hæmorrhages beneath the integument, behind the orbits, into the kidneys, etc., and finally pain and stiffness in all the joints supervening, with more or less complete ankylosis of some of the joints. In this case, within a fortnight after the beginning of the baths, without internal treatment, the joints were restored, and within six weeks all evidences of hæmorrhage had disappeared.

Surely where such marked results have been secured, our expectations for some amelioration in favorable cases of cardiac disease may be realized. If hydræmia and plethora can be influenced in this manner, we have a potent means of improving not only cases of insufficiency and stenosis, in which the relative dilatation and hyper-

trophy is dependent upon either of these conditions, but even more so in many cases of degenerated or infiltrated heart muscle, by the improved nutrition it brings to the muscle fibre.

I quote a case of aortic stenosis and insufficiency, with compensatory hypertrophy, in a man, sixty-eight years old, a sufferer from chronic gout.

The improved tone of the heart muscle as the result of baths alone could not be denied.

Similarly I quote two cases of aortic and mitral disease in two females, both married, one aged forty-five, the other fifty-seven. In the latter, although arriving with marked symptoms of failure of compensation, within six weeks the dyspnœa, œdema, and fluid in the pleural cavities disappeared, and the patient returned to her home in Boston a much-improved patient.

Ample material could be supplied by others whose experience has led them in this direction before me, but to-day, among the seven or eight thousand persons who have sought the benefits of Bad Nauheim during the past summer, Russia, France, Austria, Holland, England, Sweden, and America, all have furnished their quota of patients, whose numbers, I am certain, will in the future be much increased.

Dr. A. Schott has, within his own and his brother's experience, treated nearly three thousand cases of chronic cardiac disease, and his results, drawn from so considerable an experience, are worthy of our encouragement. With a commendable conservatism he has differed from Oertel in the mode of applying the mechanical method. Schott regards mountain climbing, however well regulated, as suited only to the latest resources after the heart has been materially strengthened by milder methods. Accordingly he resorts first to the dietetic method and baths, next to moderate passive or active movements, regulated so as to avoid dyspnœa and overaction of the heart, and, last of all, permitting his patient the daily resort to a moderate hill-side climb.

Nothing during the past years has given rise to more animated discussion and attention abroad than the dietetic-mechanical treatment of chronic heart disease, and I quote with pleasure these words of Leyden,* spoken during the past year: "I have referred to the progress of the therapeutics of heart disease in the past years, made partly by the acquisition of new medicinal remedies, but in part also by the introduction of the dietetic-gymnastic method, which in a certain sense has introduced a reform in the domain of therapeutics. Even though the controversy continue, and the results and indications have not altogether been made clear—which is not surprising, considering the complicated objects of internal therapeutics —we must, beyond cavil, confess to actual progress in this direction."

If, though, we are not yet settled in our convictions, and more experience be needed to make indications clear and results unequivocal, yet we must admit that at no time does help for the chronic cardiac invalid seem nearer at hand than now, to prolong his life and to postpone the time of failing compensation.

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* Professor Dr. E. Leyden: "Prognosis of Heart Disease," read before the Berlin Society of Internal Medicine, Deutsche med. Wochenschrift, 1889.

THE FUNCTIONS OF THE CEREBELLUM AS INDICATED BY RECENT EXPERIMENTS.

By E. D. FISHER, M.D.,

NEW YORK.

This paper was read before The New York Academy of Medicine on February 20th, 1890. It has not been put in shape for publication.—Ed.

THE ULTIMATE RESULTS OF LAPAROTOMY FOR THE REMOVAL OF DISEASED APPENDAGES.*

By H. C. COE, M.D.,

NEW YORK.

When the President made a personal request last fall that I would introduce a discussion on this theme, I declined, saying that, in the first place, I was not a laparotomist, and that, in the second, it would be more fitting for an older and more experienced observer, such as are the gentlemen who will follow me, to assume this duty. When the request came through another channel I was induced to accept the responsibility, with the distinct understanding that the discussion would be rigidly confined to a statement of anatomical and clinical *facts*, and not to a rehearsal of individual triumphs in abdominal surgery. With these triumphs we are already sufficiently familiar. The brilliant results of our honored Fellows, Dr. Wylie, Dr. Mundé, Dr. Tuttle, and many others of whose success we are proud, need no additional words of approval; their statistics speak for themselves. The indications and contra-indications to laparotomy for removal of the adnexa we have discussed time and again. The changes on the subject of conservatism have been rung so often that professional audiences are beginning to weary of them. I dismiss this phase of the question entirely, and invite your discussion of the subject as announced, which is *not* the limitations of laparotomy for

* Introducing a discussion of this subject before The New York Academy of Medicine, March 6th, 1890.

ovarian and tubal disease, *not* the number of difficult and successful operations which you have performed, but the condition of your patients a year, two years, or five years after recovery from the operation. I am aware that operators of the largest experience are often unable to trace the majority of their charity patients after they have been discharged from the hospital. It is for this reason that I have ventured to introduce this subject, in spite of my limited experience as a laparotomist, because it seems to me that the dispensary physician is in a better position to judge of the after-effects of laparotomy than is the distinguished surgeon, whose interest in the patient often ceases as soon as he has written "R" after her name in his table of statistics. As I take it, the object of the present discussion is to decide the question whether the word "recovered," as applied to these cases, is synonymous with "cured." By the "ultimate results" of laparotomy I understand that reference is made to the condition of the patient after the lapse of at least a year after the operation, when it is fair to assume that the usual symptoms referable to the sudden and artificial establishment of the menopause (flushing, metrostaxis, various nervous phenomena, etc.) ought to have disappeared, or, at least, to have become endurable. This excludes certain secondary or remote local complications, such as acute pelvic abscess, intestinal and vesico-vaginal fistula, fistulous drainage-tube tracts, mural abscess, ventral hernia, etc., which might be ascribed either to accidental causes or to some fault of the operator. We shall assume that the operation was performed strictly *secundum artem*, according to the most improved modern methods. Moreover, it will be impossible to discuss at length the results of removal of the adnexa on one side only, in spite of the interest attaching to this subject, as it would divert our attention from the main issue. Assuming that we have all had the most gratifying results follow laparotomy, as regards not only the actual preservation of the patient's life, but also her restoration from invalidism to perfect

health, I shall confine myself entirely to a rehearsal of the permanent ill-effects which may follow even the simplest and least complicated operation, leaving it to you to explain why they occur and how they may be avoided.

For convenience we may consider these under two general heads, viz.: 1, Psychical results; 2, physical results. Under the first should be included not alone the transient "psychoses" to which attention has recently been directed as sequelæ of gynæcological operations, minor as well as major, but actual insanity. I do not feel competent to express an opinion upon this subject, as the facts at my command are too few to warrant it. In a recent conversation with a prominent alienist, he stated his belief that most of the cases of insanity reported as directly due to removal of the ovaries were open to suspicion, because a careful study of the patient's history would generally show that her mind was already so affected before the operation that she would probably have become insane even if it had not been performed. On the other hand, a gentleman connected with a large asylum stated, in a paper read by him not long since, that at least ten per cent of the women whose tubes and ovaries were extirpated became insane in consequence. I am sure that this must be an exaggeration; at least I have never noted any such proportion, nor does any other author make such a sweeping statement. Careful observers do hold that there is a direct relation between the operation and the subsequent mental aberration which sometimes occurs. We ought under this head to consider the question of the "unsexing" of women by the operation, one which figured in a recent medico-legal case. We should settle this matter definitely, so far as we are concerned. The physical results of laparotomy for removal of the adnexa may be general or local. Under the former are included the various neuroses which persist for years after the operation, circulatory abnormalities such as true cerebral hyperæmia (to which I have called attention in a former paper), and various visceral disturbances, especially intestinal

troubles due to adhesions. The latter I have considered elsewhere. All who have had occasion to perform laparotomy for the second time on the same patient will agree with Martin as to the frequency of localized peritonitis in apparently aseptic cases. Peritonitis means adhesions, the influence of which on distant viscera I enumerated in a paper on "Visceral Lesions due to Localized Peritonitis." I can only touch on these points and leave their further consideration to you. I have been especially interested in the local, or intrapelvic, complications of laparotomy, yet this division of the subject is too broad to be covered by me in the brief time allotted, and, as you are already tolerably familiar with my views, I can merely outline my position, which is as follows: The persistent pelvic pain, congestion of the uterus (with or without a pseudo-menstruation), vesical and intestinal disturbances, which in a certain proportion of the cases are noted for years after removal of the tubes and ovaries, are directly due to intrapelvic indurations or adhesions, which may have existed before the operation or may be the result of localized peritonitis following it. If adhesions existed before the operation and are separated at the time, they are very apt to re-form, especially the intestinal. Small indurations around the stumps or in the broad ligaments are sufficient in themselves to cause constant severe pains, both local and reflex, and to induce a more or less permanent pelvic congestion. Even when ventro-fixation of the uterus is practised, we cannot be sure that the organ will undergo post-climacteric atrophy. Intestinal adhesions are of common occurrence; indeed, their absence is a matter of surprise, when we consider the fact that the coils of gut must always lie in direct contact either with the stumps alone or (in complicated cases) with large, raw surfaces in Douglas' pouch and on the posterior aspect of the uterus and broad ligaments. Such adhesions, even when slight, may give rise to severe and irremediable colicky pains; when more extensive, the patient is in constant danger of intestinal obstruction. When for any reason the adhe-

sions around the uterus and adnexa are separated without removal of the latter, they unquestionably re-form, either at the old or at new sites. When diseased adnexa—a pyo-salpinx, abscess, or adherent cyst—is emptied and fastened in an unnatural position, as in the abdominal wound, even if the intestines do not become adherent to it, undue traction is made upon the pelvic organs (and especially upon the neck of the bladder), thus giving rise to permanent distressing symptoms. I can only add to this brief review of the possible local complications following laparotomy, that as yet we possess no sure means of avoiding them. Rigid antisepsis; absolute hæmostasis; cauterization of the stumps, and even of all new surfaces; the routine use of intraperitoneal irrigation, and the removal of the water by siphoning alone, with the absolute interdiction of sponging; careful arrangement of the intestines and peritoneum before closing the cavity; early removal of the drainage tube—all these precautions have been adopted, and yet intrapelvic adhesions have formed, and that, too, in perfectly uncomplicated cases, in which the pelvis was absolutely free from adhesions when explored at the time of the operation. It seems to be practically impossible to predict that localized peritonitis may not develop in any given case, however favorable it may be. I dislike to dismiss this subject so abruptly, but my duty is simply to outline it, not to fill in the details. I trust that the discussion will be confined within the prescribed limits, as the introduction of outside issues will only complicate what is already a difficult question.

The following are brief notes of a few of my own cases, in which I have had the patients under observation since the operation. I might cite many more in which laparotomy was performed by other surgeons, and the patients subsequently came under my care on account of resulting troubles, but I do not think that it would be proper for me to do so.

CASE I.—Mrs. W——. Laparotomy, October, 1885. Double intraligamentous cyst with pyo-salpinx. Opera-

tion complicated by general intestinal adhesions. Cyst on right side enucleated and removed with the tube. Left tube removed and cyst drained and stitched in the wound. Good recovery. Patient still under treatment for persistent pain and dysmenorrhœa. Until recently was obliged to pass her water every hour or two. Severe vesical tenesmus. The uterus is drawn upward and is adherent to the left side of the pelvis, constant traction being thus exerted upon the neck of the bladder. I have treated her at intervals for over four years, with only temporary benefit. Her discomfort has been such that she considered the advisability of submitting to a second laparotomy.

CASE II.—Mrs. D——. Typical history of gonorrhœal salpingitis. Uterus retroflexed and firmly adherent, with double pyo-salpinx. Constant pain and severe dysmenorrhœa. Was under regular treatment for over one year without benefit. Laparotomy, September, 1887. Adhesions broken up, appendages removed, ventro-fixation of uterus. Convalescence rapid. The patient was relieved for several months, after which the pain returned. I saw her about three months ago. She had not menstruated since the operation, but had the same pain in the back and abdomen as before, with marked vesical irritation. The uterus was small and anteverted, but there was considerable tenderness around the stumps. The patient was greatly depressed and in poor general health.

CASE III.—Miss L——. The tubes and ovaries were removed in 1885 for the relief of dysmenorrhœa. Convalescence normal. She attended the Polyclinic regularly after leaving the hospital, and subsequently remained under my constant observation in various institutions until her death in 1889. Though menstruation ceased, she was never free from pain after the operation, neither was I able to afford her any relief by the ordinary local treatment, including the use of galvanism. Persistent pain had been substituted for the monthly paroxysms. It was always localized in the right side, where an induration the size of a walnut could be distinctly felt. Constant and agonizing vesical tenesmus. Repeated examinations of urine with a negative result. One of her most obstinate and distressing symptoms was more or less constant cerebral hyperæmia—not the ordinary climacteric "flushing"—which grew steadily worse. She insisted on a

second operation, which was performed by the late Dr. Hunter, and terminated fatally. The induration in question was found, but could not be removed. At the autopsy an indurated mass was discovered at the site of the right stump, which pressed the ureter, causing pyo-nephrosis of the corresponding kidney. The uterus was latero-flexed and firmly adherent. It would have been impossible to cure the patient without performing nephrectomy, and the condition of the kidney was, of course, unsuspected.

CASE IV.—Mrs. C——. Double pyo-salpinx and chronic oöphoritis, with firm adhesions. Laparotomy, October, 1888. Rapid convalescence. Both ovaries removed. The patient still menstruates regularly and with more pain than she had before the operation. There is a large mass at the site of one of the stumps. Her condition is worse than before.

CASE V.—Mrs. T——. Under constant treatment for nearly a year, and grew worse rather than better. Both ovaries adherent in Douglas' pouch, enlarged and extremely tender. Constant pelvic pains, severe dysmenorrhœa, and menorrhagia. Laparotomy, December, 1889. Diseased tubes and ovaries removed. Patient made such a rapid recovery that she was up at the end of three weeks. She has precisely the same pains as before, and has a constant sanguineous discharge, but does not menstruate. The uterus is in normal position, but is large and sensitive to the touch. Marked tenderness around both stumps.

CASE VI.—Miss O——. Was an unusually robust young woman. Laparotomy was performed for the relief of pelvic pain and dysmenorrhœa in December, 1888, diseased ovaries and tubes being removed. Convalescence normal. She was discharged apparently well, but came under my care in the clinic six months later, saying that she was worse than ever. Menstruation had ceased since the operation, but she had a constant agonizing pain in the left side, so that she was unable to walk. Constant severe frontal headache. On examination an extremely tender mass was felt at the site of the left stump, exactly similar to that mentioned in Case III. Life was a burden to her, and, with her cheerful consent, I opened the abdomen a second time about two months ago and found the mass as previously located. It proved

to be a knuckle of intestine which was firmly adherent to the left stump. It was separated with considerable difficulty, and at imminent risk of tearing the gut. When freed, the adherent surface presented an ulcerated spot the size of a quarter of a dollar, with hard raised edges. The advisability of resecting the intestine occurred to me, but I finally decided to cauterize the ulcerating surface and return it to the cavity. Thorough irrigation and drainage. Convalescence uninterrupted. Bowels moved spontaneously on the second day. When examined a few days ago all trace of the induration had disappeared and there was no local tenderness. The bowels are constipated and the patient has severe colicky pains, so that I suspect a fresh adhesion of the gut. The headache is worse than before the second operation.

Case VII.—Mrs. B——. Was operated upon a year and a half ago by a prominent laparotomist of this city. She had constant pain in the left side and dysmenorrhœa. Pyo-salpinx and diseased ovary on the left side removed. After the operation the pain was transferred to the right side and was worse than before. Has had several attacks of localized peritonitis. I first examined her three months ago. There was general tenderness around the uterus, which was in normal position. To its right there was a mass the size of a Messina orange, exquisitely tender to the touch. Diagnosis, cystic ovary and pyo-salpinx, which was confirmed at the operation a month later. The coils of intestine within the pelvis were generally adherent to one another and to the tumor; intestinal adhesions along the line of the former incision. The adhesions were thoroughly separated and the tube and ovarian cyst were removed, leading to profuse hæmorrhage which was checked with difficulty. Irrigation and drainage. Rapid convalescence without elevation of temperature. The patient now complains of the same pain on the right side; on examination a tender induration can be felt at the site of the right stump, pressure upon which causes a paroxysm of pain which lasts for an hour.

Case VIII.—Mrs. A——. Double pyo-salpinx removed in September, 1889. Convalescence normal until the second week, when there was a transient elevation of temperature, accompanied by severe pain in the right groin. Vaginal examination revealed an induration around the stump, which is still present after the lapse of

six months, the patient having constant pain in the right side, which prevents her from attending to her household duties. She had no metrostaxis until three weeks ago, when she reported to me on account of profuse flow. I found the uterus small, in normal position, and quite tender to the touch; the induration to its right was smaller than it was two months before, but was still very sensitive. The patient was greatly depressed because she was no better.

I present the above facts without comment, and could easily double and treble the number of cases. If with my limited experience I have observed so considerable a proportion in which laparotomy is not followed by permanent benefit, at least so far as regards the relief of pain, those operators who number their cases by hundreds could, if they would, add much to our knowledge in this direction. Unfortunately, there is a singular reticence on the part of surgeons with regard to the ultimate results of their operations, provided that these are less successful than they expected.

So far as I am concerned, whenever this subject is introduced I shall never cease to insist upon this truth, which I have repeatedly demonstrated to my own satisfaction in the examining room, at the operating table, and in the dead-house—that recovery from laparotomy is *not* synonymous with cure.

THE RELATION OF ERRORS OF REFRACTION AND INSUFFICIENCY OF THE OCULAR MUSCLES TO FUNCTIONAL DISEASES OF THE NERVOUS SYSTEM.*

By D. B. ST. JOHN ROOSA, M.D., LL.D.,

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About thirty-three years ago, when Donders began to make known in Graefe's *Archives* his great discovery, the fruit of some years of labor in the light of the ophthalmoscope, that asthenopia was chiefly the result of an error of refraction whose nature he had himself clearly demonstrated, he found the medical profession engaged in proving that it was due to defective action of the external muscles of the eye. It was then taught by the highest authorities, that the cause of asthenopia was to be sought in a spasmodic contraction of some of the ocular muscles. There were practitioners, as Donders remarked, who had the courage to divide these muscles for asthenopia, and he proceeds to say, that this is a melancholy page in the history of operative surgery.† But these practitioners are to be forgiven; they had gone as far as our knowledge of physiology would allow. The source of the accommodation of the eye for vision at different distances, had not then been discovered, nor had its mechanism been explained. The retina, however, had been abandoned as the *fons et origo mali*, and we were nearing a solution, when the ills of asthenopia were ascribed to muscular action

* Read before The New York Academy of Medicine, March 20th, 1890, in a discussion on The Relation of Peripheral Irritation to Disease.

† "On the Anomalies of Refraction and Accommodation," p. 272.

and not to the retina, even if the wrong muscles were attacked.

Any proper discussion of so-called ocular reflexes, resulting from strain of the eye, must begin with a definition of asthenopia. Perhaps that of Donders is the best:

"The eye has a perfectly normal appearance, its movements are undisturbed. The convergence of the visual lines presents no difficulty. The power of vision is usually acute, and nevertheless in reading, writing, or other close work, especially by artificial light, or in gloomy places, the objects after a short time become indistinct and confused, and a feeling of tension comes on, especially above the eyes, necessitating the suspension of work. The person so affected now often involuntarily closes his eyes, and rubs his hands over the forehead and eyelids. After some moments' rest he once more sees distinctly, but the same phenomena are again developed more rapidly than before. The longer the rest has lasted, the longer can he now continue his work." *

But Donders went no further than to say that the asthenopia thus described depended upon one of two causes: either upon a short eyeball, hypermetropia; or upon some constitutional disturbance, such as occurs in a marked way in paralysis from diphtheria, and prevents the use of the muscles of accommodation. This latter form has been often confounded with that which Donders said was caused by hypermetropia. It may exist coincidentally with hypermetropia, usually of a low degree, or with weakness of ocular muscles, and it is an accommodative asthenopia. But an error of refraction is not at all the cause of it. Exhaustion of the nervous system, hereditary or acquired, from various sources, is at the bottom of this kind of asthenopia. It often depends upon incurable general conditions. But true accommodative asthenopia is curable by means of correcting glasses. The former variety is sometimes called American asthenopia.

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* Loc. cit., p. 259.

It is more common in our country than in France, perhaps than in Germany and England. At least one Frenchman thinks it is more frequent in New York than in Paris, because New York men and women are not so well nourished by good and well-cooked food and sound red wine, as are the corresponding class in Paris. I am not prepared to dispute this opinion.

It is important to bear in mind this distinction between accommodative asthenopia occurring in neurasthenics, women with uterine disease, malarial subjects, those recovering from typhoid fever, and so forth, who, at the same time, like the rest of the world, have slight refractive errors and insufficiencies, and pure accommodative asthenopia in healthy subjects, caused either by high degrees of hypermetropia, astigmatism of even moderate degree, great weakness of the internal recti, unequal refraction of the two eyes, or the like.

Although Donders was the discoverer of the nature and the cause of asthenopia, he had no idea of its frequency. While he recognized the fact that headaches, melancholy sensations, and a long train of evil symptoms might result from asthenopia, he did not know how frequently these symptoms were caused by ocular fatigue. In his time the public were somewhat discouraged in getting medical aid for the relief of these symptoms, and readily gave up the use of their eyes for near work, and turned to agriculture or idleness as a palliative for what could not be cured.

Donders found in a shortened eyeball the chief cause for asthenopia; as he says, "hypermetropia is usually at the bottom of asthenopia." If he had known as much of the effects of astigmatism as he and Knapp had shown us of its nature, he would have said hypermetropia *and* astigmatism.

At this very time, Graefe was in the midst of his investigations of muscular asthenopia. Donders was careful to describe muscular asthenopia also. He believed that it depended chiefly on insufficiency of the internal recti muscles. Up to this day Donders' classification has been

pretty generally accepted, and all writers speak of accommodative and muscular asthenopia. But Donders underrated the importance of astigmatism in producing asthenopia, and, scientific and wise as he was, he was so overshadowed by the great genius and authority of the pioneer in modern ophthalmology, Graefe, that he laid undue stress upon muscular asthenopia, although his work removed a great deal of what I believe was not only then, but is now, a fictitious importance ascribed to this latter condition. Even then he called attention in an uncertain way to the important fact that errors of refraction were practically fixed conditions, while muscular insufficiency must be as variable in the eyeball as in the leg or hand. But Donders' work stopped much of the cutting of external ocular muscles and narrowed the field of muscular asthenopia. In reading the work of the great Dutch physiologist in the light of subsequent years, it seems as if his courtesy restrained him from speaking his full mind about the *ignis fatuus* of asthenopia dependent upon insufficiencies of ocular muscles. Graefe admitted, at a very early stage, that the combined action of the recti interni for accommodation for the near point is extremely different under physiological conditions in different persons. He might have added, in the same person.*

Matters as to asthenopia, both in the old and new world, rested for a number of years about where Donders left them. Although he speaks of gloomy feelings being changed to those of hope and exuberance, in consequence of a relief from asthenopia by wearing glasses, and adduces several graphic cases of this kind, he did not fully appreciate the great relief that the practice of ophthalmology, in the light of his discovery of the nature of hypermetropia and astigmatism, was to bring to thousands upon thousands of suffering men and women, much less did he dream that the origin of nearly every form of disease of the nervous system was to be found in an error of

* Archiv, B. iii., Ab. 1, p. 308.

refraction or an insufficiency of ocular muscles. It was reserved for America to claim that not only asthenopic affections were due to errors of refraction and accommodation and muscular insufficiencies, but, to quote the exact words of the author, "difficulties attending the functions of accommodating and of adjusting the eyes in the act of vision, or irritations arising from the nerves involved in the processes, are among the most prolific sources of nervous disturbances, and more frequently than other conditions constitute a neuropathic tendency." *

But the definition of asthenopia became more comprehensive, the resulting headaches and twitching of the lids were more fully described and recognized by ophthalmologists everywhere. Blepharitis and styes and tarsal tumors were added to the list of these consequences. † But until about 1874 the evil consequences of uncorrected errors of refraction, were supposed to be limited to the area directly in connection with the eyeball. There was no claim that asthenopia, except by mental depression, extended beyond this region in its evil effects.

At first it was claimed that chorea as well as epilepsy, although sometimes dependent upon organic lesions, were generally functional nervous affections, under which head they are classed. And even now Dr. Stevens says:

"The anatomical lesions of the brain or of the cord are sometimes coincident with chronic chorea, it is true, but that there is necessary or causative relation between the two conditions has not yet been shown. Indeed, it is much more probable that the anatomical lesions of the brain and cord are among the results of chorea, or of the irritation causing it, and that they are not in any respect the original sources of irritation." ‡

Of late this claim has been somewhat modified, and great pains are now taken to state that, when epilepsy and chorea are mentioned, there must be careful discrimina-

* "Functional Nervous Diseases," George T. Stevens, M.D., 1887, p. 21.

† Roosa: Transactions International Congress of Ophthalmology, 1876.

‡ Loc. cit., p. 88.

tion between functional and organic chorea and epilepsy,* and that only the former are proper subjects for operation. In the early papers we hear very little, if anything, of the differences between chorea and epilepsy dependent upon organic causes, and the functional forms. Dr. A. L. Ranney, who is well known as a disciple of the promulgator of the doctrine of the ocular origin of nervous disease, even now speaks of chorea as always a functional nervous disturbance. He says that it is doubtful, to his mind, if any changes exist in the brain or spinal cord with chorea.† The same author admits that epilepsy may have an organic cause. But it is apparent that he believes that this is exceptionally true and that epilepsy is also a functional disease.‡

When chorea, epilepsy, and insanity were all classed under the head of functional nervous disease, it was at first claimed that hypermetropia was the chief influence in the production of these functional nervous diseases. It was then deemed sufficient to correct the hypermetropia or the astigmatism. In a paper read before this Academy,§ it is plainly stated by Dr. Stevens, that "chorea is a functional disturbance of the nervous system, which may give rise to organic lesions, and which arises from irritation dependent upon anomalous refraction of the eye, and in a very large proportion of cases to hypermetropia." But of late years, say from 1886 or 1887, the muscles are said to play the chief part in causing the functional diseases, and operations upon them are necessary for the cure. A new nomenclature has been promulgated for the purpose of defining muscular insufficiencies, and operations upon these muscles have largely supplemented or taken the place of the correction of errors of refraction. Thus we have returned in a circle to the position in which Donders found Graefe, and the

* Journal of Nervous and Mental Diseases, November, December, 1889.

† "Lectures on Nervous Diseases," p. 494.

‡ Loc. cit., 474 et seq.

§ Transactions of The New York Academy of Medicine, vol. ii., p. 440.

ophthalmologists preceding him. Asthenopia, from which nervous diseases of all kinds are now said to result, is chiefly dependent upon muscular insufficiencies and is generally to be relieved by operations upon these muscles.

It now becomes necessary to make rather a long quotation, in order to show what is claimed will follow from an adoption of the general principle, that functional nervous diseases depend chiefly upon faulty action of the eye.

"I think it not unreasonable to look for the future advance in medical practice along two great lines. That advance along one of these lines was begun when Jenner, recognizing the fact that the human subject may be made sterile to the development of certain organisms when once it has been preoccupied by the presence of the same or a similar class of organisms, introduced vaccination as a preventive of one of the greatest scourges of the race. In our day an army of investigators, well trained and well equipped, is exploring the realms of the minute in search of the micro-organisms which, in their invasions in swarming myriads within the human body, threaten to destroy it by their devastations. It is not impossible that against the inroads of the various organisms whose incursions constitute typhoid and typhus, scarlet fever, and measles, cholera and yellow fever, barriers may be erected which shall in large measure protect against the class of maladies which now destroys so large a proportion of the population of the world. The advances which have already been made along that line are, doubtless, but the earnest of that which is to come.

"Along the other line we may look for as great achievements. The class of maladies which has been during all time relegated to the tender mercies of fetichism and superstitious notions of cures by drugs having no relations to the origin or the nature of the disorders themselves, will ere long be regarded as irregular phenomena resulting from well-defined causes of irritation, which causes must be sought for principally in the direction of difficulties in

the performance of necessary functions. With the removal of such difficulties we may look with confident expectation to the cessation of the peculiar irregularity which constitutes the special form of nervous disease. Through such means we may reasonably expect that the great class of functional nervous troubles, of which epilepsy and insanity, neuralgia and hysteria may be regarded as representatives, will be nearly as effectually guarded against as is small-pox by vaccination." *

The quotation just made, is an extract from the paper that led to the formation of a commission of experts, to investigate the subject of the cure of nervous diseases by glasses and tenotomies.

It was supposed at one time by some of the writers who have discussed this subject in the medical journals, that insufficiencies of the ocular muscles, except of the interni, were never discussed until the discoveries of 1876 and 1880; but reference to the writings of Graefe,† the nephew, Loring, and others, shows that this was incorrect, and that even latent insufficiencies of all the muscles, were recognized years before this.

It was Alfred Graefe also who clearly laid down the rule, lately so often ignored, that errors of refraction and accommodation should be corrected before noting muscular insufficiencies, and this for the simple reason that muscular insufficiencies are often caused by uncorrected errors of refraction.

These novel views as to the causation of functional nervous diseases, have now been subjected to years of very thorough investigation, accompanied, on this side of the water especially, with much acrimonious disputation. In Europe they have excited little attention. A learned commission has sat upon the whole subject, and, although its report is unanimous, and bears, on the whole, rather heavily against the value of the discovery of a great prin-

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* Stevens: New York Medical Journal, p. 428, vol. xlv., April 16th, 1887.
† Saemisch: "Handbuch," Band vi., 4. Theil, p. 216.

ciple of cause and cure, it cannot be said to have closed the subject, for in the discussion that followed the report of the commission, one of its members seems not to be quite certain of the propriety of an adverse report. In his remarks he lays great stress upon the delicate instruments and manipulations which have characterized these curative operations, and he also said that, "not being a neurologist, he did not feel competent to judge of the therapeutic effect of these operations."

From the editorial remarks of a journal conducted by another member of the commission, we may fairly conclude that he also did not fully agree with what was supposed to be a unanimous report. A leader in this journal says: "Now, what does this prove, and does it prove anything? It would seem to prove that Dr. Stevens' method is a therapeutic measure valuable as an adjunct, and that in certain cases where toxic medication is unwise, it constitutes one more means of alleviating suffering." *

The commission, however, unanimously expressed the opinion that the method of Dr. Stevens does not afford a sufficient degree of relief to patients suffering from chorea or epilepsy to warrant its adoption or recommendation to the Neurological Society, as a means of cure, or as the sole therapeutic measure." † In the debate that followed among the neurologists, there was a minority which did not fully agree with the report.

It is evident, that there is still such a state of unsettledness in the minds of quite a fraction of the medical profession upon this subject, as to justify further discussion. For this reason I have accepted the invitation of the President of the Academy, to present my views upon peripheric irritation as a cause of disease, especially with reference to irritations of the eye.

The basis of argument by which chorea and epilepsy

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* New York Medical Journal, November 30th, 1889.

† Report, p. 32.

were considered to be due to errors of refraction and accommodation, has, in my opinion, been long since shown to be entirely fallacious, although in the monograph upon this subject from which I have already quoted, great stress is laid upon these tables. The following is an example of one of them :

Of one hundred and eighteen cases of chorea,* seventy-eight are said to have been hypermetropic; thirteen had hypermetropic astigmatism; five had mixed astigmatism; six were myopic, and eleven had myopic astigmatism.

This table, and also the similar tables that are furnished with reference *to errors of refraction* in epilepsy and migraine, are of no more importance than if they stated that of one hundred and eighteen persons suffering from measles or a broken thigh, such a proportion were hypermetropic and so forth. It has been shown by myself,† by Ely ‡ in his " Examination of the Newly Born " (correcting the observations of Jaeger, who had supposed the infantile eye to be myopic), by Randall,§ and others, that only a very small proportion of adults or infants, say seven per cent, have absolutely emmetropic eyes. We did not know this when Donders made his discoveries, although Donders knew that a low degree of hypermetropia was exceedingly common, and he did not reckon such as likely to cause asthenopia. Indeed, he did not consider it to be an abnormal condition.

Stimulated by the claim made in 1876, which I have quoted, that functional diseases of the nervous system were due to hypermetropia more than to any other one cause, my investigations of adults who had never suffered from asthenopia, and whose eyes were placed under the full influence of atropia, were first made in 1877. They were followed by others, and all agree in showing that, exclud-

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* Stevens : " Functional Nervous Affections," p. 91.

† Transactions American Ophthalmological Society, 1878.

‡ " Archives of Ophthalmology," vol. ix.

§ American Journal of the Medical Sciences, July, 1885.

ing the myopic, nearly all the world is hypermetropic. If all the world be myopic or hypermetropic, how can it be in reason claimed that these conditions in themselves cause the functional diseases of the nervous system? At least the advocates of such a doctrine would be obliged to admit that other causes must act in conjunction with these deviations from an almost unknown normal standard, to produce any grave disease.

It was demonstrated by these papers, and by C. S. Bull, that people could be perfectly well and have all the errors of refraction and accommodation that were said to cause chorea and epilepsy. These tables of errors of refraction occurring in epilepsy and chorea were thus stripped of all their value, and the arguments resting upon them fell with them. Without fairly admitting this, the tables were still published, but the argument was now changed to meet the new facts confronting the new theory. In a paper read in 1880* attention was called to the fact, said to have been discovered in 1876, that insufficiencies of the straight muscles of the eye, by causing want of co-ordinate action, might cause nervous disturbance in the same manner as, or in conjunction with, refractive lesions. Then the formula was altered, and now functional nervous diseases are dependent upon anomalous conditions of the refraction and accommodation, *or upon imperfect action of the muscles of the organ*, "more than any other causes combined."

It seems to have been thus tacitly conceded, that errors of refraction alone, are not the chief source of functional nervous disease, but this was not said, although the argument was transferred to the muscles of the eye, to muscular insufficiencies. It is the doctrine that nervous disease is caused by these insufficiencies, and is relieved by ocular tenotomies, that I now wish to combat.

Before I follow this doctrine of the dependence of neuroses upon want of ocular muscular equilibrium, to its

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* Stevens: Transactions Medical Society, State of New York, 1880.

legitimate ending by statistics showing how few people have such a muscular equilibrium, I will read the main outlines of a table which shows what people who come to an oculist for glasses actually suffer from, and how few of them have anything but accommodative asthenopia. Considering the assertion that, "more than all other causes combined," epilepsy, chorea, and other functional nervous diseases depend upon conditions of the eye, is it not remarkable that so few of them consult an ordinary oculist? These tables were prepared for me from my private case books, by Dr. Frank N. Lewis.

A table showing the symptoms complained of by patients who came to have glasses fitted:

Asthenopia	1,445
Not seeing well in the distance	428
No history*	325
Blepharitis, styes, chalazia, etc.	265
Headaches	213
Not seeing well for near work	211
Glasses unsatisfactory	191
Conjunctival irritation, trachoma	178
Pain in the eyes	149
Diplopia	43
Muscæ	39
Lachrymation	31
Sent by a neurologist to learn the refraction	20
Dizziness	15
Twitching of the lids	11
Photophobia	5
Blur before one eye	5
Chorea	3
Sudden loss of vision	3
Twitching of the muscles of the eye	2
Orbital neuralgia	1
Dilated pupil	1
Total number of refraction cases	3,584
Total number of patients examined	6,455

* Those having no history were chiefly cases of presbyopia or myopia, in which no asthenopia was complained of.

The refractive conditions actually found are shown in the following table:

	H.	M.	H. ast.	M. ast.	Mixed ast.	Refraction different in the two eyes.	Pr.	Muscular insufficiency.
Asthenopia	528	81	263	86	27	86	49	23
Not seeing well in the distance	35	166	42	104	26	20	..	..
No history	30	89	30	41	8	5	82	1
Blepharitis, styes, chalazia, etc	111	19	53	11	3	12	3	..
Headaches	53	12	71	26	9	26	5	4
Not seeing well for near work	33	..	31	13	4	..	..	..
Glasses unsatisfactory	6	31	38	45	6	3	17	..
Conjunctival irritation and trachoma	95	21	24	11	2	..	17	..
Pain in the eyes	28	23	20	22	4	6	6	..
Diplopia	10	..	1	..	1	..	..	3
Muscæ	17	2	2	2	1	1	..	..
Lachrymation	12	2	6	1	..	..	6	1
Sent by neurologist	6	1	1	1	..	..	3	1
Dizziness	5	2	3	..	1	2	..	1
Twitching of the lids	2	1	3	1	..	..	..	..
Photophobia	1	2	1	..	..	..	..	..
Blur before one eye	2	..	1	1	..	..	..	..
Chorea	3	..	..	..	..	..	..	..
Sudden loss of vision	..	1	1	..	..	..	..	..
Twitching of the muscles of the eye	..	1	..	1	..	..	..	..
Orbital neuralgia	1	..	..	..	..	..	..	..
Dilated pupil	..	..	1	..	..	..	..	..

These patients came to me, not because they had general nervous disease, but because they had symptoms beginning plainly in the eye, or in its appendages or adjacent to them. There I think may be found a true guide in diagnosis. It will be remembered, that in accordance with the theory that the causes of chorea and epilepsy, and so forth, are to be found in the ocular refraction or muscles, it is not necessary that the eye or its appendages should manifest any symptoms at all. The condition of the eye is to be revealed by skilled observations as to the refraction, and delicate tests as to muscular equilibrium. These causative conditions may be completely concealed until thus demonstrated. Patients who never knew that they had an ocular symptom, are told that all their troubles proceed from their accommodative and visual apparatus. This is not in accordance with the usual condition

of things. A splinter in the tissue, a foreign body of any kind in any part of the body, whatever reflex disturbance it may give rise to, will usually give local symptoms also, and thus call attention to its origin. In this, I do not believe the eye, ear, nose, and throat are any exceptions. If we have actual reflex disturbances of the general system from the eye, besides those disturbances, asthenopia in one of its many forms will show itself. I do not believe that a refractive error or a want of muscular equilibrium that causes no local disturbance, will, except in highly exceptional cases, if ever, cause any constitutional affection.

It is not possible, in such a paper as this, to give even the outlines of the individual cases that demonstrate in detail, and unanswerably, the failure of this theory to account for the origin of nervous diseases. If *falsus in uno, falsus in omnibus*, be good logic, every oculist's case book will overthrow the doctrine of the ocular origin of nervous disease. Chorea has been entirely recovered from years before any correction of an error of refraction occurred. In other instances, one eye has never been used with the other, and yet most active careers, requiring most profound, patient, and exacting mental labor, have been successfully carried on, marred only by slight asthenopia. Again, vertigo said to depend upon want of co-ordination of the ciliary and recti muscles, has been recovered from, while the want of co-ordination remained just the same. Every oculist of wide experience sees many such cases. They utterly destroy the theory that any discovery has been made of wide adaptation, which places refractive defects at the basis of general disease of the nervous system.

I could speak also of the essays upon this theme, which gravely fill pages of print with the most primary details as to accommodation and tests of vision, details to be found in every text book upon diseases of the eye, but which are described as being something new in connection with the great discovery of ocular conditions, "more

than all other causes combined" as giving rise to nervous disease. I might also mention the anxiety produced in the laity about their eyes, which has caused presbyopes who have no accommodation of any disturbing kind, to submit to disabling of their vision with atropine for two weeks, in order finally to get a pair of glasses, which any oculist would prescribe, and correctly prescribe, in fifteen minutes by the clock. If this dictum which we are discussing be true, most physicians should become oculists. Very little other knowledge than how to test the refraction and the muscles, and how to cut the latter, will be needed, if the predictions as to the importance of this discovery are fulfilled.*

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* The views of those who believe that migraine is always to be ascribed to ocular defects, if those defects are found, are well presented in a recent lecture by Dr. Seguin (New York Medical Journal, April 5th, 1890). He says: "The proportion of subjects of migraine who have ocular defects is amazing; very nearly all have either errors of refraction or muscular insufficiency, or both combined. In 1882, I received from Dr. G. C. Savage, of Jackson, Tenn., a very courteous letter challenging me to furnish a case of migraine in a person with normal eyes. I have not yet met with one, though I have been told of two or three by oculists in whom I have confidence. Of course the statement as to the invariable concomitance of eye-faults with migraine presupposes that the patients have been examined thoroughly—*i.e.*, under the full effects of atropine for refractive errors, and by Stevens' method for muscular insufficiency. I regret to say that there are still oculists of good standing who examine the eyes of headache cases in the most careless way, ordering glasses without having used atropine, and ignoring the muscles altogether. This has happened under my observation in New York within six months."

All this argument falls to the ground, when we learn that thousands of people have all these defects and yet have no migraine. I have also seen many persons with migraine who were no better after a careful correction of astigmatism and muscular insufficiencies, although some are much relieved by glasses, especially cylindric glasses. Dr. Seguin adds: "Another argument in favor of the ocular origin of migraine is that other remarkable fact, that in many persons of both sexes the attacks diminish and cease between the ages of forty and fifty years. It is at this period that the power of accommodation becomes exhausted and a large part of the unconscious strain which has been going on from early youth is removed."

This cannot be the explanation, for it is just after presbyopia comes on, that astigmatism often becomes very troublesome, especially astigmatism of a moderate degree. I know of many cases where asthenopia, blepharitis, and so forth, first appeared when glasses were needed for fine print, and the

It having been pretty thoroughly established that errors of refraction among well people, are the rule and not the exception, the next thing naturally is to determine how many well people—that is, well as to asthenopia and nervous symptoms—have what is called muscular equilibrium, or, to use a modern nomenclature, have *orthophoria*, or parallel eyes.

I have recently investigated these conditions of muscles, which are said to be the prominent, if not the chief, factors in causing neuropathic conditions, and I present the table herewith. The investigations were made by one of my staff, Dr. Deynard, of the Manhattan Eye and Ear Hospital, with the phorometer, with strict attention to the details laid down in the recent writings upon this subject. I personally have never examined the cases. Dr. Deynard has been perfectly untrammelled in his investigations.

One hundred and three persons who did not have any trouble with their eyes that they knew of, who read and wrote and sewed, without headaches or asthenopia, who had no vertigo, chorea, epilepsy, hystero-epilepsy, or insanity, were selected as objects for testing. Eighty-three were aural patients attending the Manhattan Eye and Ear Hospital; six were friends who came with them; eleven were physicians; one was a music teacher; one was a servant, and one a detective. Seventeen, or sixteen per cent., were found to have muscular equilibrium; eighty-four, or eighty-one per cent., had a want of muscular equilibrium, so-called heterophoria; of these, twenty-seven, or twenty-six per cent., had deviation outward, exophoria, insufficiency of the interni, and seventy-four, or seventy-one per

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spherical ones proved insufficient. The cessation of attacks of migraine can hardly, in the light of this fact, be due to the let-up in the accommodation incident to middle life. Dr. Seguin falls into the error, now so common, of ascribing headaches and so forth to the eye, simply because errors of refraction are coincident with the neurosis.

Many neurologists have unfortunately not kept up with recent accessible ophthalmological literature. They are working upon lines that were given up by oculists ever since the actual percentage of ametropia was discovered.

cent., exophoria in accommodation; sixteen, or fifteen per cent., had deviation inward or esophoria, insufficiency of the externi; seven had esophoria in accommodation; eleven, or ten per cent., had hyperphoria, a tendency of the right or left visual line upward; twenty-four had hyperphoria in accommodation. A re-examination of five of these patients all showed a change in the muscular examination from that found at first. This is an important observation, since it proves, as asserted by Starr and others, that the muscular power in the same eyes is not fixed, but variable.

Hence it is seen that any series of cases founded upon certain ocular insufficiencies, cannot be said to be proven when the existence of these insufficiencies is shown, for they may exist in connection with entirely healthy nervous systems just as errors of refraction may. The value of observations founded on such tables is now no more than of those founded on errors of refraction. Not only does all the world have faulty refraction, but very few people possess equilibrium of the ocular muscles.

The capacity for great intellectual work with great muscular insufficiencies, is well shown by the following extract from a letter relative to the perception of distance by Sir William Rowan Hamilton, Astronomer Royal of Ireland,* printed in his biography: "Though I habitually see a double universe, yet a marked improvement has taken place within the last few weeks in my power of seeing single. This I attribute to my having lately, for the first time in my life, bought a stereoscope and used it at leisure. A friend within a few minutes' walk of me has long had a stereoscope apparatus, but years elapsed before I could catch the effect at all. With each eye separately I saw a good relief, but it is 'two years ago' that I first was able to see that tertium quid which is the true result of the stereoscope, and certainly it greatly aston-

*I am indebted to my friend Dr. Dennet for this quotation from the life of this eminent savant.

ished me" (written by Hamilton to De Morgan at fifty-eight).

Hamilton also states that on the evidence of his friends he did not squint, and his photograph shows that he did not do so in any marked degree.

The study of the causation of disease is certainly a very important one, but it is almost as puzzling at times as the theological problem of the origin of evil in the world. Yet some things we do know. If a man lives in a malarial swamp and intermittent fever attacks him, or if he drinks water polluted with typhoid bacilli and gets typhoid fever, or if he is exposed to a case of small-pox and breaks out with this disease, or if after prolonged exposure to wetting he is attacked with acute rheumatism, we have no difficulty in saying as to where the cause of his disease is to be found. But no philosopher will conclude that cholera is caused by insufficiency of the ocular muscles, because a large proportion of those seized with this disease, as is certainly true, have such an insufficiency. The true philosopher will recognize in nervous maladies a series of causes acting together, and at the bottom of them all, in this country at least, will be found that ill-defined condition of which we know so little, but of which we shall know more, called nervous exhaustion.

To go back to errors of refraction for a moment. Another fact should be noted in this discussion. Contrary to what is stated by Ranney, hypermetropia is constantly outgrown in the development of the eyeball, as every ophthalmologist knows. A hypermetropic eye may even become myopic. Glasses needed at twelve and fourteen are discarded without asthenopia at twenty, and so forth.

The inconvenience of uncorrected errors of refraction, especially of high degrees of hypermetropia, and of even inconsiderable degrees of astigmatism, can hardly be overestimated with reference to their influence upon the head and the face, and upon the spirits of some patients, but their influence generally seems to stop there, except in very rare cases in neurotic subjects.

It is not an uncommon experience, in malaria and after recovery from typhoid fever, to find accommodative asthenopia, which is only relieved when quinine has cured the malaria, and time and hygiene have restored the shattered nervous system of the sufferer from the *fièvre nerveuse.*

Those who believed that in the eye, "more than in all other causes combined," is to be found the cause of functional nervous disease, would have been now on surer ground if they had stood by the first declaration, that errors of refraction were the cause, and not taken refuge in want of muscular equilibrium.

A great deal of muscular insufficiency depends upon uncorrected astigmatism. Alfred Graefe and Loring were right. We can say very little as to what constitutes a muscular insufficiency until the refraction, and I say especially the astigmatism, be corrected. A thorough use of atropia, or, better still, an examination by Javal's ophthalmometer, will often cause us to correct a troublesome insufficiency by cylindric glasses and not by prisms, much less by a tenotomy.

There is a large class of neurotic patients who may or may not have high degrees of errors of refraction, generally, however, they have only low degrees of hyperopia or hyperopic astigmatism, whose accommodative asthenopia, like their headaches, depends upon hereditary faults and predispositions. These neurotic families seek, now in one panacea and now in another, cure they will never get. Palliation is all there is for them. Neurotic they are born and neurotic they will die, although for a time one set of symptoms may be transferred to another. Like Dr. Loring's case which I have before alluded to in this Academy, "their eyes have got all right, but it has gone to the stomach." I know of a neurotic family where one member has had a dozen operations upon his eyes, but he is as neurotic as ever. Another has had all her troubles transferred from her ovaries to her eyes, by excessive attention to them. She goes about discoursing

upon advancements and divisions of recti muscles; but sure that without her oculists she would have died. A third member, with myopic astigmatism in one eye and myopia in the other, and the difficulties of presbyopia coming on, is nearly harassed out of his domestic peace by the urgent desires of his more fortunate relatives that he should seek relief in ocular tenotomies. Other patients, who have never suffered from asthenopia until they began to undergo treatment for nervous disease by tenotomies, wander over the earth seeking to have a double vision removed, of which they once knew nothing, and yet complaint is made by the author of the doctrine I am combating that, after months and even years of attendance and several tenotomies,* patients are withdrawn from treatment while defects of great importance are known to exist, which by continuous effort might be removed.

Donders' argument, that in errors of refraction and not in muscular insufficiencies would be found the chief cause of asthenopia, was not put very plainly, but I undertake to say that not in the muscles but in the eyeball, in the refraction, the sources of ocular reflexes are to be found, such as occasionally exist, just as a foreign body in the ear may occasionally cause epilepsy, or a decayed tooth cause spasm of accommodation. The condition of the muscles is unstable, as has been pointed out; and, as my tables show, what is a man's power in his ocular muscles to-day, may be another to-morrow.

These cases of epilepsy and chorea dependent upon peripheric irritation are entirely exceptional, while asthenopia is real and common. We have no reason to hope, from anything that has yet been shown, that there is any large class of functional constitutional nervous diseases caused by peripheric troubles, and, as I have said, whatever there are, are to be sought for not in the muscles but in the eyeball. The conclusions to which I have

* New York Medical Journal, April 16th, 1887, p. 427.

come, as a result of twenty-six years of work in ophthalmology, with a careful consideration of the writings and works of others, including those who believe that functional nervous disease is due to the eye "more than to all other causes combined," are as follows:

1. The eyeball of the human race is very rarely in what might be defined as an emmetropic condition.

2. Perfect equilibrium of the ocular muscles is by no means a common condition, even among persons of sound health and without asthenopia.

3. Defects in these two states by no means necessarily produce even local disturbances, such as are comprehended under the term of asthenopia, inflammation of the edges of the lids, and so forth, although high degrees of hypermetropia, moderate degrees of astigmatism, and all cases of mixed astigmatism are apt to do so, sooner or later.

4. Asthenopia depends chiefly upon two sets of causes —nervous exhaustion and uncorrected errors of refraction.

5. In estimating the influence of these defects great stress should be laid upon the general condition, and the line sharply drawn between asthenopia due to exhaustion and that to faulty refraction.

6. Nothing has been added essentially to Donders' discovery of the fundamental cause of accommodative asthenopia, except that astigmatism forms a more important factor in its etiology, muscular asthenopia falls into the background, while the definition of asthenopia has been much amplified by ophthalmologists.

7. The origin of a considerable class of such diseases as chorea, epilepsy, and hystero-epilepsy has not been found in errors of refraction nor in insufficiencies of the muscles of the eyeball.

The observing general practitioner of our city is able to test the truth of this latter proposition as he looks among the neurotic families and individuals of his acquaintance, and observes whether or not the use of glasses and the

performance of ocular tenotomies, are beginning to lessen the cases of chorea and epilepsy. Certainly a discovery which it was predicted would vie with those of Jenner and Lister has been proclaimed with sufficient fulness and clearness, and has been acted upon in an arena large enough to allow us by this time to judge whether or not it is an advance in rational and scientific therapeutics. I do not believe that it is, but I believe that the theory of the ocular origin of nervous disease, by distracting attention from the evident, and in some instances preventable causes of neuroses, has delayed our progress, and inflicted serious damage to the reputation of our profession for accurate observation and philosophical conclusions.

ON THE RELATIONS OF PERIPHERAL IRRITATION TO DISEASE, AS MANIFESTED IN THE THROAT AND NOSE.*

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NEW YORK.

During the past few years some eminent specialists in laryngology have attributed, as I believe, too much importance to irritations of the throat and nasal passages in the causation of disease. This position was almost to be expected when we reflect how prone the medical mind is to assign a prominent place to the particular organ whose diseases are familiar in the etiology of most human ailments. Add to this the fact that it has been proven that nasal obstructions are unquestionably, at times, the efficient factors in producing an outbreak of hay fever or of asthma, and we have a sufficient reason to appreciate why surgical methods for the relief of these conditions became familiar and widespread. While we should see in this march evidence of progress and improvement, we should also be prepared to note what is evident exaggeration and excess.

Up to the time that Daly, Roe, Sajous, Bosworth, and others showed the nasal factor in the production of hay fever, this affection was integrally allied with a neurotic habit and the presence of pollen in the atmosphere. After the recognition of the fact that the nasal organ was frequently the seat of diseases which caused peripheral irritation, and thus led directly to hay fever, it was deemed

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* Read before The New York Academy of Medicine, March 20th, 1890, in a discussion upon The Relations of Peripheral Irritation to Disease.

essential to employ proper means to get rid of these diseases.

The affections referred to appeared to be of different kinds; sometimes, in the opinion of careful observers, being of the nature of neuritis of the peripheral nerves of the pituitary membrane, as shown by the numerous areas of painful sensibility. Again, the turbinated bodies were in a state of notable hyperæmia or hypertrophy, or the septum was thickened or deflected, or polypi filled the nasal fossæ, or an adenoid vegetation clogged the post-nasal space. These were regarded as conditions which were strictly morbid and to be removed, with the legitimate hope of ameliorating or curing the hay fever. It soon became a routine practice to make use of the galvano cautery, and in several successive sittings to cauterize all the attainable sensitive areas on the pituitary membrane. Or, if sensitive areas were not discovered, it was judicious to remove any existing obstructions to easy nasal respiration by means of the saw or trephine. Now, what has been the result? At first, and during a period of two to three years, it was not uncommon to read of the radical cure of hay fever being thus effected. Then a few accurate observers began to doubt these wonderful cures, and to notice that after a time the hay-fever victims were again sufferers, although in many instances their symptoms had been much ameliorated and they were hopeful for a time of a complete cure. Unfortunately, the cures were few when the cases were watched and followed up with impartial judgment. Theories were not wanting to account for the benefits which accrued to the patient from operation. Among the most seductive of these, I would refer to that of vaso-motor paresis of the large veins of the turbinated bodies, which caused a peculiar hyperæmia almost pathognomonic of the disease. This vaso-motor paresis is occasioned, as is stated, by the intranasal obstructions. When we remove the latter, we cure the former condition, which was a fruitful source of sneezing, nasal discharge, conjunctivitis, and cough. This interpre-

tation is, I believe, in a few rare instances true. In many it is in great part, if not entirely, erroneous. There are instances of hay fever in which cauterization, sawing, and trephining are wholly negative as regards their effects. Inasmuch as the operations have been undertaken frequently when it was simply the personal conviction of the operator which caused the nose to be occluded, and not the actual fact of its being so, we can appreciate that the patient was injured, if not relieved, by unnecessary and useless operation.

Now, I do not desire for a moment to impugn the motives of such operators; I merely desire to direct attention to lack of judgment.

Admitted, they say, that hay fever depends upon a neurotic habit and a specific irritant, still the latter would not take effect if the nose were in a healthy condition, and they proceed to discover with absolute certainty this intranasal disease. Occasionally they are correct in their interpretation: the nose is diseased and requires surgical treatment. In many instances the morbid state of the nasal organ is merely a resultant of morbid processes situated elsewhere, and it will be remedied from this broad standpoint only by judicious medication.

Finally, there are instances in which the amelioration of the case may depend, in some degree at least, upon the proper local treatment of the nasal membrane; but this treatment should be that of soothing or altering the condition of the soft parts by suitable topical applications, and not the ablation or resection of portions of organs supposed to be diseased, when in reality they are healthy.

Much of what has been said as applicable to hay fever is also applicable to hay asthma. Perennial asthma should now be considered. Can this disease—so difficult to treat successfully, so varied in its manifestations, of such different origin—be cured frequently by the removal of a nasal obstruction? Certainly and unhesitatingly I answer, no, it cannot. Peripheral irritation in the nose is without much doubt a predisposing cause of asthma in a certain

proportion of cases; in a very moderate percentage should it be regarded as a direct and efficient cause of this disease. Asthma, as we know, may be occasioned by any number of conditions which are of the nature of peripheral irritations. A disordered stomach, a loaded rectum, a painful ovary, a decayed tooth may precipitate the attack. In like manner an irritated or obstructed nose may produce this effect. I have seen many cases already in which the removal of mucous polypi or enlarged turbinated bodies from the nasal passages seemed to result in a cure for a time; but the cures were not permanent; the attacks of asthma returned sooner or later. It may be that they were ameliorated, that their intensity was diminished, and their frequency lessened; but the patient was not permanently cured.

In this connection I recall the circumstance of a distinguished practitioner of another city, who dined at my house some years ago, and who told me in confidence at the time of his great relief from asthmatic attacks by the extirpation of nasal mucous polypi at the hands of a distinguished specialist of New York. Forty-eight hours later I was seated in an upper bed-room of the Fifth Avenue Hotel, caring for my confrère, who had had as bad an attack of asthma as I ever saw.

In a paper read by me before the American Climatological Society, in June, 1889, I stated that during the preceding year I had seen and treated about thirty cases of asthma due to different causes. Among these there were six in whom the asthma might be fairly ascribed to the existence of intranasal obstruction. Of these six cases, five were due to hypertrophy of the turbinated bodies in conjunction with more or less deviation and thickening of the nasal septum. The sixth case was one of nasal polypus. In all six cases operations on the nasal passages were undertaken. The nose was rendered pervious, and the patients experienced much comfort as a result of the operations. As regards the effect on the asthma, it was noted that in four cases the asthma was at least temporarily

improved. In two cases there was no marked relief. In the histories elsewhere reported in this paper there were many cases in which asthma existed, and yet at no time was there any appreciable nasal obstruction. Further, I have seen a large number of cases, several of which I have operated upon successfully, in which there was pronounced nasal occlusion, and at no time had there been the slightest tendency to asthma. Such cases, of course, can be readily explained by saying that the special neurotic element was lacking in them, and on this account the attack of asthma failed to occur. That may be true, but it was not always evident upon examination of the patients, and in some cases the nervous temperament was highly predominant.

These facts, I regret to say, are not wholly in accordance with the statement of Dr. Bosworth, who says that a large majority, if not all cases of asthma, are dependent upon some obstructive lesion in the nasal cavity. Against this we have the affirmation of Dr. Shurly, who writes that "daily experience overwhelmingly shows a very large number of cases of intranasal disease in persons of neurotic habit, with a very small proportion of cases of associated asthma."

"We are going much too far," says Dr. A. H. Smith, "when we seek to establish it as a rule that asthma is to be cured by operative procedures within the nose." As Dr. Daly affirms, it is unquestionably true "that there is a certain proportion of cases of asthma that can be cured by putting the naso-pharynx in a healthful condition." And it is also true that some cases of asthma can be cured solely through the instrumentality of the cautery, the saw, the rongeur forceps, or the nasal trephine. These are the instances in which it is obvious that the permeability of the nasal passages is essential in order that the function of respiration may be carried on properly, and that the respiratory surface may be protected, and especially against the evil effects of constant or intermittent mouth-breathing. In the latter cases, the lungs do not receive a sufficient supply of air, and this fact may be cor-

roborated by listening to the chest of one who alternately breathes through the mouth and the nose. Moreover, the nose is a filtering apparatus, besides providing the proper amount of moisture to the inspired current of air, and if it be interfered with the foreign bodies floating in the atmosphere, instead of being arrested on its lubricated vibrissæ, penetrate directly into the lungs and cause, necessarily, irritation and distress.

Therefore, whenever an operation is required to render the nose permeable, and particularly when there is a tendency to, or confirmed, asthma, it should not, if possible, be delayed or neglected.

Frequently, however, the nasal occlusion is due to a congestion of the pituitary membrane, which in these cases may be greatly diminished or wholly relieved by the medicinal agents which are employed for the purpose of restoring the freedom of the pulmonary circulation. Again, crusts, scabs, mucus accumulate in the nasal passages at times, and promote an inflammatory process which can be temporarily relieved by a suitably formulated spray. I have known the hypersensitiveness of the pituitary membrane to be greatly diminished at times by repeated applications of carbolic acid with glycerin and vaseline. Indeed, in this way I recall two cases of asthma of nasal origin in which I effected a temporary cure.

To show the difference of opinion in regard to the intranasal treatment of asthma, and particularly by cauterization, I would add that in the discussion following the reading of Lazarus' paper on this disease at the Berlin Medical Society, "the general consensus of medical opinion seemed to be that the number of cases in which asthma was benefited by cauterizing the nasal mucous membrane on the turbinated bones was extremely small." *

In summing up this subject, I will quote at present, with some modifications to render it more appropriate to the actual discussion, from a paper written by myself and

* Annual of the Universal Medical Sciences, 1888.

read in 1886 before this Academy of Medicine. The title of this paper is "Respiratory Neuroses of Nasal Origin." * In it I write: "There are, without doubt, a great number and variety of affections, mainly neuroses, under the direct influence of morbid intranasal conditions; still, at the present time we should be careful not to exaggerate their importance, and, when and wherever we find a neurosis of the respiratory organs, immediately reach the conclusion that the nose occasions it." There is much truth in the observations of the past few years in regard to the predominating influence of a hypersensitive nose, but there is also a tendency to exaggeration in this regard, and we should be careful to exclude other efficient causes of asthma before admitting its nasal origin and instituting severe or prolonged treatment of this organ with a view to cure the possible secondary affection. The nose is naturally a sensitive organ, and many times I have produced epiphora, cough, spasmodic closure of the glottis, moderate in amount, and slight dyspnœa, by the mere passage of instruments or simple nasal examinations, when the result of my further investigation was to show that no nasal trouble of importance existed. This is no new observation, and I am conscious that all of you are familiar with it. In reading the histories of clinical cases we shall be disposed to take a one-sided view, unless we secure the sobering influence which comes from extensive reading and larger experience. The only way, I believe, in which we can avoid error at present in this seductive question, is to search carefully for other cause or causes of asthma; and when none are discovered, then, and then only, we should admit the nasal origin of the neurosis, and institute treatment in accordance with this conviction. I agree entirely with Loblinski, who says: "If there is really a nasal affection, it should be treated according to its character, and not on any far-fetched theory of its etiological importance." †

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* New York Medical Record, p. 119, 1886.

† Medical Record, May 27th, 1887.

This being my opinion, I must reluctantly separate myself from any such radical position as that upheld by my eminent colleague, Dr. F. H. Bosworth, of New York, who writes that he has seen no case in which intranasal disease was not found as a predisposing cause of asthma. And, again, to quote him textually, he has "never known a case of asthma to occur in other than an obstructed lesion of the nose or upper air passages."*

I fear I have left myself but a very short time to allude briefly to other matters of much interest. Ocular symptoms and disease not infrequently originate from hypersensitiveness of certain intranasal areas. These may be photophobia, asthenopia, conjunctivitis, epiphora, contracture of the eyelids, etc. All these I have met with. On two occasions I cured my patients by intranasal treatment when that directed to the eye had wholly failed.

The recurring nocturnal cough of children, when no pulmonary complications exist, is due frequently to congestion of the posterior portion of the lower turbinated bodies, which produces contact and pressure on the septum at this level. In view of the unique vascular and free nervous supply of this region, the anatomical explanation of this fact is sufficiently obvious. Judicious topical treatment of the nose alone relieves this troublesome symptom. Cough is produced by peripheral irritation engendered by acute or chronic rhinitis.

It is also frequently dependent on nasal polypi, enchondroma, exostoses, and nasal deflections. In the naso-pharynx it may arise from naso-pharyngeal catarrh, or from adenoid tissue at the vault of the pharynx. Hypertrophy of this structure gives rise to congestion of the lower naso-pharynx, and thus increases the sensory excitability.

Palpitation of the heart may become, it is stated, a prominent symptom of the hypertrophy. Chorea, especially of the facial muscles, is not infrequently present. Headache is often complained of, and spasm of the

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* American Journal of the Medical Sciences, September, 1888, page 256.

glottis may occur and become a very troublesome symptom. Tornwaldt has found in some instances that the bursa situated in the naso-pharyngeal region becomes diseased and causes, as he believes, hyperæmia and hypertrophy of the nasal mucous membrane. He also reports cases of asthma which were apparently dependent upon cysts in the pharyngeal vault, and which were cured by incision and treatment of these cysts.

These facts are much at variance with Bosworth's explanations as previously reported. In fact, Bosworth confesses his "entire inability to reconcile this statement either with his own clinical observations, or with his knowledge of the physiological and pathological processes of the upper air tract."

It is interesting to remark that Hack and Tornwaldt both claim that they have in frequent instances cured obstinate cephalalgia by treatment of the naso-pharyngeal disease. Hack advises, indeed, that in all instances of severe headache a direct inspection shall be made of the upper air tract, in order to discover any possible source of disease in this region. Of course, if it be detected (he says), it should be judiciously treated, and such treatment will be followed by cure of the head symptoms.

It is pardonable, I trust, to remark in this place that one could but sympathize with the poor patient who, under these circumstances, had also a special ocular disease; and, perhaps, also a passing attack of eczema, with evidences of general neurasthenia.

The outcome of such a situation would be four specialists and one general practitioner to help restore the unlucky mortal to a better state of health.

To resume the tenor of my discourse: In common with many physicians, I believe that inflammation of the larynx, trachea, and bronchi is often due to the extension of a chronic inflammatory affection from the naso-pharyngeal space, or to the fact that the secretions make their way down the pharynx and enter the larynx from above. This opinion is directly opposed to that of Dr. Bosworth,

who claims, on page 525 of his work on "Diseases of the Nose and Throat," that he "does not suppose that the secretion from the nasal passages or the pharynx ever makes its way into the laryngeal cavity under any circumstances."

At page 550, Bosworth, in writing of hypertrophy of the pharyngeal tonsil, forgets his previously expressed views for the moment, and unfortunately commits himself to the following statement: "Cough is present in certain cases, especially if there is much secretion which has made its way down the pharynx into the larynx, exciting a laryngitis." I should be glad to know which opinion Dr. Bosworth really adheres to, and, if to the one first expressed, how he explains the development of the secondary laryngitis, unless it be from peripheral irritation in the nasal passages.

While I fully recognize that subacute or chronic laryngitis is frequently a secondary condition, and that the nasal passages are the first to become diseased, yet I claim that there are numerous instances in which the laryngitis is unquestionably the primary affection, and should be thus treated.

Follicular pharyngitis often occasions cough. This disease accompanies, or is secondary to, chronic nasal catarrh. The enlarged follicles become painful at times and unduly susceptible to irritation. Frequently the current of inspired air over these inflamed structures produces a short, dry cough, or a constant inclination to "hem." This disease is also not infrequently accompanied by marked dysphonia and weakness of the voice, which is apparently due to secondary involvement of the nerve trunks whose course lies along the pharyngeal walls. The nature of the nervous affection in these may be either a neuritis or atrophy through pressure from surrounding infiltration of the soft parts.

Enlarged tonsils are occasionally attended with paroxysmal and distressing cough or sudden attacks of dyspnœa, which closely resemble the suffocative attacks

of laryngismus stridulus. Sometimes the attacks of dyspnœa are so intense that a sudden fatal termination is feared. They may occur at meals or at night during sleep. According to some authors, the sudden attacks of dyspnœa which afflict children are evidence rather of some rachitic disease of the bones of the skull (craniotabes) than of enlarged tonsils. Irritation of the large lingual papillæ or lymphoid tissue situated at the base of the tongue, when present in excess, occasions obstinate dry cough, dysphonia, and a sensation of stricture around the neck. The latter condition interferes somewhat, at times, with the powers of deglutition by pressure on the epiglottis, or simply owing to peripheral irritation. These different conditions can be relieved or cured by suitable treatment, sometimes purely medical, sometimes with surgical aid especially directed to the parts involved.

In terminating this brief summary of the subject which has been given me, I would like to add that, had time permitted to read it, it might have been made much fuller and more elaborate. As it is, I trust I have written sufficiently to indicate the points upon which discussion is desirable.

In this place I would remark, by all means let each one of us look for peripheral irritation in the nose and throat when it seems proper so to do, as a source of disease elsewhere, and when discovered let us treat it rationally, judiciously, and effectively; but not allow ourselves for one moment to believe that the nose or throat, any more than any other organ of the body, is always diseased and should always be treated, either medically or surgically. Keep the general economy in good working order, with some special attention to each organ in a suitable degree and when it is really required, and, take my word for it, there will be much less to do for the rhinologists and for other exclusive specialists, and the majority of people will enjoy more peace of mind and health of body.

To quote from a late editorial of the London *Lancet* *

* February 22d, 1890, pp. 412 and 413.

and apply it to the subject of debate, I may well add that the study of throat disease "has proceeded along very narrow lines, and pathological theories have been formulated upon insufficient and often erroneous data and made the basis of universal treatment." Some of these views have already "been proved to be wrong, while others are, on the surface, of such an improbable character that they should be received only on the production of irrefutable proof of their correctness."

Would that I could bring to bear on this subject Dr. Goodell's brilliancy of interpretation, and conclude my paper with watchwords analogous to those he has furnished the gynæcologists with as broad helps to diagnosis. We may, however, paraphrase one of them with advantage, which is this:

Always bear in mind that men, women, and children have some organs outside of the nasal cavities.

In conclusion, it is well to bear in mind that evidence is forthcoming that the abuse of intranasal treatment may carry with it unpleasant consequences. In corroboration of this, I quote from Dr. Rethi,* of Vienna, who enters a protest against the indiscriminate use of the cautery and snare in the treatment of intranasal disease. Though neuroses arising from disease of the turbinated bodies may often be cured by operation, he has noticed that in some cases they are increased, and that reflex conditions may even be originated by the cicatricial tissue which forms after operations.

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* Vide Medical News, March 8th, 1890, p. 258.

REFLEX NEUROSES.*

By M. ALLEN STARR, M.D., Ph.D.,

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THE active part of the nervous system is the gray matter. The white fibres merely carry impulses to and from it. This gray matter is collected into three quite distinct masses: (1) the spinal and medullary centres—*i.e.*, those from which the spinal and cranial nerves arise; (2) the basal ganglia; (3) the cortex. In the entire vertebrate species it is possible to assign to these distinct masses different functions: to the spinal and medullary centres, reflex action; to the basal ganglia, automatic action; to the cortex, conscious and voluntary action. In dealing with reflex disturbance it would, therefore, at first appear that we were to be limited in our discussion to affections of the spinal cord and medullary centres. These must certainly be considered. Yet we are not thus limited. For each lower system is under constant control from the one above it; hence the spinal (reflex) activity, being the lowest, is dominated both by the basal (automatic) activity and by the cortical (voluntary) activity. Hence, disturbances of reflex action may be brought about by anything which either increases the normal inhibitory action of the higher centres or removes that action, as well as by disease of these reflex centres themselves. A prompt motor response to a sensory stimulus is the action which should normally take place when the reflex centres are called into play. Sneezing, winking, swallowing, cough-

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* Read before The New York Academy of Medicine, March 20th, 1890, in a discussion upon The Relations of Peripheral Irritation to Disease.

ing; the secretion of alkaline saliva in the mouth and acid gastric juice in the stomach when any foreign body is put into these cavities; the act of vomiting when that body is an active irritant, or of increased peristalsis of the intestines if the foreign substance reaches it; the flushing of the skin with blood when exposed to changes of temperature; the regular respiratory and cardiac movements, and the vascular tone—these, as well as the so-called tendon and skin reflexes, are examples of reflex activity. Without such reflex action the balance of supply and demand in the human economy would be impossible and life itself would cease. A power of quick and appropriate response to normal or abnormal irritation is a natural power of the lowest masses of gray matter. I cannot find any evidence from the study of disease that the exercise of this power is in any way detrimental. This power may be interfered with in several ways. Strychnine and its coefficient drugs will increase it. The result is manifest (1) by an unnaturally strong response to a weak irritation; (2) by an abnormally long and continued response to ordinary irritation. The spasms on slight touch and the opisthotonos with rigidity in a guinea-pig poisoned by strychnine are examples of both. In man, tetanus furnishes a good illustration; but the extraordinary state of nervous irritation, the starting and jumping at little noises, the flushing on slightest irritation, the cramps produced by heat, the extreme uneasiness and hyperæsthesia present in spinal meningitis, are equally good examples of increased reflex activity due to direct irritation. Reflex activity may be suspended by violent and sudden irritation. Injuries to the abdomen or head may arrest all reflex activity and cause death in man as well as in a frog, as described by Goltz. It may be impaired by an excess of moderate irritations coming simultaneously. Just as the conscious attention cannot be given at one time to several subjects and elicit simultaneously appropriate responses to all, so reflex centres may be embarrassed by numerous simultaneous stimuli and fail to react to any one in proper degree. The con-

dition subsequent to severe traumatic concussion is an illustration.

When we seek for illustrations of either increased or decreased reflex excitability from spinal disease, it is evident that we must allude to very serious diseases. The reflex mechanism is too important and too stable to be affected by little irritants or by slight causes. If it were not, the numerous, inevitable stimuli consequent upon an active life would constantly put that life in danger. How few of the severe accidents which constantly occur really affect in any marked degree the reflex nervous system! How many serious irritations, traumatic, painful, distressing, fail to cause a single spinal nervous symptom! The surgical wards of a hospital offer little field for the observation of reflex nervous disturbance—happily for the inmates. In the lower animals such affections rarely exist.

Turning now to the other cause of disturbance of reflex function, defective control by higher centres, we at once enter upon a broader field. Arrest of reflex activity is rare and needs little consideration. It may be produced voluntarily, as one may prevent the knee-jerk by intention, or inhibit the act of swallowing, as in hysterical dysphagia. It may be produced involuntarily, as when cerebral hæmorrhage inhibits respiratory or cardiac action; or extreme fright causes death in the same way. Increase of reflex activity from central causes is common. The act of control must be constantly exerted in order to preserve the balance of power and the proper harmony of action in the nervous system. Whether we start from a consideration of moral conduct or of the plantar reflex, we reach the same conclusion. And in central control a slight defect produces widespread and serious results. A man who has once abused a confidence will not be implicitly trusted, though he live for a hundred years. A small clot in the brain will set all the motor, vaso-motor, and sympathetic centres in such a state of instability as to impair all confidence in

their action. A hemiplegic who has apparently recovered still has the increased knee-jerk. This may not in any way impede the action of the reflex centres. That is carried on exactly, accurately, satisfactorily, and its action does no harm. The emptying of the bladder as soon as it is full is a perfectly normal, satisfactory act on the part of the lumbar spinal cord, and active incontinence of urine is not a symptom that it is diseased. It is a symptom that impulses calculated to restrain the act till an appropriate time and place offers are prevented from reaching that normal reflex centre. There must be a proper harmony and succession of reflex acts with some definite order for the nervous system to fulfil its whole duty. And any defect in that order may cause effects which in other conditions would not be defects.

If these patent facts of physiology be accepted, I think we must conclude that in those diseases known as reflex neuroses, in which there is no real defect in reflex action, but only an undue or inharmonious reflex activity, the actual origin of the disease must be either in an excess of activity in the lower centres or a defect of control in the higher centres. As we have seen, an excess of activity is commonly called forth only by very severe causes; while a defective control may come from slight ones. Phenomena comparable to tetanus or strychnine poisoning are rarely, if ever, seen in reflex neuroses. In the various reflex neuroses in which peripheral irritation is the supposed cause of disease, it is my opinion that when peripheral irritation is the actual cause, Nature indicates it by attracting attention to the seat of irritation by discomfort or by pain.* And when, as in very many cases, the source of the irritation is by no means evident, is only found, or supposedly found, by extraordinary effort, the probability is largely in favor of the assumption that the reflex neurosis is not produced by any direct peripheral irritation, but is the manifestation of a slight or serious

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*See Medical Record, January 1st, 1890.

defect of control by higher centres, due to their impaired nutrition and consequent impaired activity.

Theories are attractive, but misleading. Experience is a better guide. In my own experience, I am sure that a very large majority of the patients who present symptoms of reflex neuroses have been exposed to influences which undermine the strength and nutrition of the *entire* nervous system; have serious symptoms, not only of a so-called reflex character, but of defective central control; are to be regarded as suffering from functional or organic disease of the highest, most complex, most highly developed portion of the nervous system; and are not merely the victims of irritation in some peripheral organ.

We know that long since the notion of a reflex paralysis—such, for example, as a general paraplegia from a slight wound of the hand—has been abandoned, and these cases are to-day explained more satisfactorily upon the theory of mental shock rather than spinal irritation.* That they are rare must be conceded. Two surgeons of this city, of the largest experience, have told me that they have never seen a case. We are coming rapidly to the notion that the symptoms in the so-called condition of spinal concussion are really due to the same state of mental shock. The highest, most delicate, most easily affected centres of the nervous system, those which are the last to arise in the process of evolution, are, as Hughlings-Jackson has pointed out, the first to fail in dissolution, are the easiest affected by sudden and unexpected mental irritation. How else can we explain the fact that, in a railway accident, passengers who are asleep rarely suffer from after-effects? Passengers who are awake suffer the immediate alarm and the subsequent symptoms.

The phenomena of hypnotic suggestion have proved conclusively that many so-called spinal neuroses are really due to defective cerebral control. It is not necessary to call such defect hysteria. Mental shock and mental worry

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* Leyden: "Reflex Lahmung," Volkmann's Klin. Vorträge, No. 1.

may produce such an unstable state of the highest centres that their inhibitory action is removed, allowing lower centres to manifest unusual activity. In such cases, then, the actual manifestation may perhaps be in a lower centre, but the actual seat of disease is not necessarily there. It is in the higher centre, which controls the lower.

Let us take, for example, a case of so-called uterine reflex neurosis. The irritation from the organ reaches the lower spinal cord. If its centres are really excited to activity, we should expect such symptoms as occur in a spinal meningitis, viz.: hyperæsthesia of the legs and cramps and spasms of the calves, vesical and rectal tenesmus, and active incontinence of urine. But these are not the symptoms found. You do find pain and a dragging sensation in the back. This does not cause any manifestation of true spinal reflex activity. There is no evidence of such activity. It does produce a wearing discomfort, appreciated consciously; received, then, in the highest cerebral centres; exhausting them, and impairing their control over the lower, especially the vaso-motor, centres. Hence the headaches, general nervousness, flushings, feelings of heat, etc., symptoms of general instability of the lower centres, *not* of the lumbar cord centres themselves in particular.* It is a mistake to call this a reflex neurosis. It is a general nervous exhaustion, due to a peripheral pain. The actual spinal reflex centres have no part at all, or very little part, if any, in the complex of symptoms.

I think the same is true—*mutatis mutandis*—of eye strain. Let an eye be actually strained by defective vision, defective accommodation, or muscular asthenopia, and the result is increased winking, watering of the eye, feeling of inability to use it, pain, heavy eyelids, possibly conjunctival hyperæmia; and in an animal under strychnine, nystagmus, or twitching of the ocular muscles, a

* Goodell: "The Abuse of Uterine Treatment," the Medical News, December 7th, 1889.

phenomenon rarely seen in man—never seen, in my experience, from the so-called strain of the ocular muscles. Every other muscle in the body, if much overstrained, will become irritable; the knee-jerk can be increased by exercise; the biceps twitches spontaneously after a weight is raised. Who has not felt the tremor of an overstrained set of muscles? Why is it that, in eye strain of the external ocular muscles, nystagmus is never seen? A serious strain of the intra-ocular tension—such as occurs in astigmatism and hypermetropia—gives rise to discomfort, to voluntary effort to strain the eyes in using, to effects, in other words, upon *not* the centres governing ocular motion, but upon the conscious voluntary centres of the cortex, upon the centres of highest evolution, most delicate, most susceptible to slight effects. The result is cortical exhaustion, similar exactly to that produced by uterine or ovarian pain, or by pain anywhere, or by mental shock or mental worry.

Take, lastly, the so-called reflex convulsions from genital irritation in boys—due to phimosis, or concretions behind the prepuce, or to masturbation. Such convulsions occur only in neurotic children. They are always preceded by mental irritability; they never occur without some evidence of cerebral irritation. It is rarely that any such evidence of spinal irritation as frequent micturition or cramps in the calves of the legs, attends them. The irritation, then, is acting upon the entire nervous system. The weakest part yields first. That part is the part which controls the other parts. The child is not suffering from reflex irritation. Irritation at the periphery is the cause only in part; a weak nervous system is the potent predisposing cause. And, unless this be strengthened at the same time that the peripheral irritation is removed, there will be a recurrence of the symptoms from some other apparently trivial cause, and he who treats these will never cure the patient. The same thing is true of genital irritation in adults. Spermatorrhœa, seminal emissions, atrophy of the testicles, exist unnoticed in many persons

until attention is called to them, the mind dwells on them, the higher conscious emotional nervous system is irritated by worry, and then the symptoms of so-called sexual or spinal neurasthenia develop. The trouble is *not* spinal, *not* reflex, it is cortical. Thus physiological investigation and careful clinical study combine to prove that while peripheral irritation may produce nervous symptoms, it is not directly through the lower centres, it is not by exciting reflex action. It is rather by producing exhaustion of the highest conscious centres which control the entire nervous mechanism. The largest engine is governed by a very small steam crank. A little motion of that will produce very great motion elsewhere. If the engine is whirling itself to destruction, do not attempt to stop the fly-wheel, do not catch hold of the binding straps, but turn off the steam by the little crank.

In any case of so-called reflex neurosis do not be satisfied by merely removing supposed peripheral irritation, but increase the nutrition of the entire system, give it food and drink, strengthen it by every possible means, baths, exercise, rest; regular, not hurried work in many directions, not in one only; and by tonics, to enable it to exert all its powers of control, and the peripheral effects will disappear of their own accord.

In the presence of a case of neurasthenia with peripheral symptoms, of chorea, or epilepsy, or hysteria, with sources of peripheral irritation present, go back of the apparently important symptoms to those of real causal moment, and the therapeutic results will be more satisfactory and more enduring.

THE FALLACY OF THE SO-CALLED HOT-AIR TREATMENT OF PHTHISIS.*

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AMONG the many systems of treatment for pulmonary tuberculosis, none have excited more widespread interest than the so-called antiseptic methods which are the immediate outgrowth of the discovery of the tubercle bacillus by Koch. One of these methods originated in Germany about four years ago, and is employed to a considerable extent to-day in this country. It is based upon the fact that heat of a degree slightly above the body temperature destroys, or renders innocuous, the tubercle bacilli, so that, if the germs can only be heated sufficiently *in situ* by the inhalation of superheated air, they will cease their mischief. To accomplish this result several kinds of heaters have been devised. Krull, in Güstrow; De Renzi, in Naples; Weigert, in Berlin; Halter, in Halle; and Sears and Richardson, in Boston, have all invented new apparatus for hot-air inhalation.

This new treatment has been employed in a large number of cases of tuberculosis. Dr. Bowie, of London, reported last year, in the *Lancet*, that he had prescribed inhalations of air with the Weigert apparatus at 120° to 140° F. (48.8° to 60° C.) for a phthisical patient, with the effect of increasing the cough and expectoration for the first four weeks, although the general condition as to appetite, sleep, etc., improved and the bacilli diminished.

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* Read before The New York Academy of Medicine, April 3d, 1890.

No medicine was given. Bowie omits the mention of the character of the diet or hygienic surroundings, and does not fully describe changes in the physical signs. He says that he has advised this treatment in several other cases, and has seen improvement in each.

Schmid recently reports several incipient cases of phthisis treated by Weigert's apparatus, in which the sputa lessened, and appetite and body weight increased.

Trudeau tried the Weigert method faithfully with four patients, who were under daily treatment by two sittings, of two hours each, for periods varying from one to four months. He used air at 392° F. (200° C.) in the apparatus. Two patients became discouraged and gave up the treatment. Another, during a four months' course, seemed to have less dyspnœa and perspiration, fewer râles, with more nearly normal respiratory rhythm and slight gain in weight. During the progress of these cases, the sputum of the patients was injected into rabbits at regular intervals of five weeks, and its virulence as affecting these animals underwent no diminution whatever. In addition, cultures of the bacilli were made, and Trudeau arrived at the following conclusions: "1. The therapeutic value of hot-air inhalations in phthisis is doubtful. 2. The evidence obtained by the bacteriological study of the cases presented does not confirm the assumption that inhalations of heated air can either prevent the growth of the tubercle bacilli in the lungs of living individuals or diminish the virulence of this microbe when it has gained access to them."

Louis Weigert reports, in the *Medical Record*, five cases treated by hot-air inhalations quickly raised to 320° F. (160° C.). These five patients were under treatment from one to six hours daily for periods varying from one to five months, and they exhibited a certain amount of improvement in both physical signs and general symptoms.

The Weigert apparatus was exhibited by Dr. A. L. Stern before the Section in the Practice of Medicine of the New York Academy of Medicine, on February 19th,

1889. This apparatus consisted of a double copper cylinder with a connecting tube, mouth piece, and valves. The air within the cylinder is heated by a Bunsen burner or lamp to 482° F. (250° C.), though Weigert himself admits that the temperature when the air reaches the mouth piece is "actually only about 320° F. (160° C.)." Dr. A. Jacobi, having had practical experience with this apparatus, at a subsequent meeting of the Academy declared it to be very clumsy. It undoubtedly was both cumbersome and expensive.

A few days ago a cheap pamphlet was thrust beneath my door, entitled "Proofs that Consumption Can be Cured," and which advertises, with various eulogistic letters and newspaper notices, the merits of a cheaper and improved form of "Perfection Hot-air Machine," patented and for sale by the "Dr. Louis Weigert Co.," which is recommended for "consumption, asthma, chronic bronchitis," and a host of nasal and laryngeal affections!

Dr. Stern claimed for the hot-air treatment of phthisis that "complete cures" result, with "total cessation of cough" and expectoration, with disappearance of fever and night sweats. He said, also, that after four months of treatment the bacilli totally disappear. He also stated that in Vienna one hundred and fifty cases of phthisis had been treated by the new method, fifty of which had been reported.

In commenting upon the claims of Weigert and Stern, the *Journal of the American Medical Association* says that "all this reads like a fairy tale," and continues, "there is but little doubt that this method of treating phthisis will be extensively tried."

Dr. A. Jacobi, at a meeting of The New York Academy of Medicine held on June 20th, 1889, reported that he had been induced to give the Weigert apparatus fair trial at Bellevue Hospital, with the result of temporary improvement in the cases treated; but he was convinced that this improvement was due to rest and better air than the patients had had at home, rather than to any influence of the

hot air. He expressed the opinion that little, if any, of the heated air ever reached the alveoli, and said that in the Weigert apparatus used the air fell from 300° F. (148° C.) in the cylinder to nearly the temperature of the body by the time it reached the mouth. Two of his patients had severe cough and vomiting as a direct result of the inhalations.

Dr. E. L. Shurley, of Detroit, at a meeting of the American Medical Association, June 27th, 1889, reported the use of the Weigert apparatus in twenty cases of phthisis. He considered only eight of these as having had a fair trial. Of these eight, only two showed any kind of improvement, and the remaining six had to stop the treatment within one or two weeks, on account of sore mouths, dyspnœa, pains in the chest, nausea, or similar reasons.

Sears applied the hot-air treatment to four cases of phthisis. He employed an apparatus devised by G. L. Kingsley, of the Harvard Medical School, which must have been more efficient than others, judging from the results; for one patient became salivated, and the mouth grew so sore as to require intermission of the treatment. In another patient constant cough was excited. The third patient grew much worse, and died from hæmorrhage several days after cessation of the treatment; and the second and fourth cases both had hæmoptysis for the first time in their history.

Prof. Dr. Kohlschütter reported to a medical society in Halle-on-Saale the successful use of hot-air inhalations in several cases of pulmonary tuberculosis, and he spoke with much enthusiasm of the new treatment, claiming that by it a few more bacilli are destroyed each day, while the symptoms of cough, expectoration, etc., diminish.

The alteration in symptoms reported by those who have used the hot-air treatment is no argument in its favor. None of these changes have been of a decided type, and every one accustomed to deal with cases of tuberculosis in its various stages is familiar with the frequent slight changes in the cough, or sputum, or appetite, or body

weight that naturally occur in the course of three or four months of the disease, and which are readily explained by variations in diet, hygiene, environment, etc. How often the faith that many such patients repose in any new treatment works a temporary improvement in their condition! The improvement was never very decided, in the best authenticated cases, and in many instances the patients were simultaneously taking creosote and other remedies. Moreover, the lung gymnastics, necessitated by deep inhalation, may, in certain cases, have been of benefit by expanding the lungs more fully, thus increasing tissue change in various ways quite independently of any additional heat. The only observers (Sears and Trudeau) who have made a really thorough report of the relative number of bacilli present while the patients were under treatment, agree in failing to find any material diminution. The mouth would doubtless become much drier and more burned than it does, were it not for the fact that the salivary glands are stimulated to increased secretion by the irritation of the warm tube in the mouth.

The observers above cited all seem to have omitted to perform any simple experiments to determine whether the heated air really conveys its heat to the lungs or not, but contented themselves with speculation.

Dr. Sears says that, in other words, "a hot poultice has been applied to the lungs, and an active hyperæmia induced," and he alludes to the danger of the rarefied hot air favoring hæmorrhage from vessels weakened by ulceration or aneurismal dilatation, while the patient himself is exposed to ordinary atmospheric pressure.

Halter, who first suggested the use of hot-air inhalation for baking bacilli, has calculated that air heated to 122° F. (50° C.) is so rarefied and expanded as to equal in density air inhaled at an elevation of 1,350 metres, and air at 392° F (200° C.) equals in density air at a height of 4,880 metres. Yet he disadvises forced inspiration of the hot air! In reviewing these statements, Lazarus very justly inquires how a patient is to draw such rarefied air into

the lungs against the ordinary external atmospheric pressure, unless by extraordinary respiratory effort.

While the body remains under normal atmospheric pressure it is a physical impossibility to fill the lungs completely with rarefied air (*i.e.*, superheated air), or even to exert a negative pressure sufficient to forcibly rupture either normal or weakened vessels. A very simple calculation of the superficial area of the chest, multiplied by the normal atmospheric pressure bearing constantly upon that surface, shows the impossibility of exerting sufficient force by the chest wall to sustain the external air, were the lungs filled with air sufficiently rarefied to rupture blood vessels. The air would be condensed within the lungs to the normal pressure. Let any one convince himself of this by closing the mouth and nostrils, and then trying to expand the air in the lungs by attempting to expand the chest.

Weigert and Kohlschütter not only claim that the hot air penetrates to the deepest alveoli, carrying great heat with it, but that the exhaled air has a temperature ranging as high as 140° F. (60° C.), and that this hot air destroys the bacilli in some instances, and lessens their virulence in others.

Krull claims that heated air acts like a cataplasm, dilates the lung capillaries, increases the rate of the circulation, promotes tissue metamorphosis, diminishes local symptoms, and increases strength. This claim is the more remarkable since he employs air heated but little above the body temperature. Krull also refers to the danger of inducing hæmorrhage by the rarefied hot air. He now claims to get complete cures by inhalation of moist air at 112° F. (44° C.) for three-fourths or one-half hour twice daily. He says that there is no doubt about the hot air carrying its heat to the depths of the lungs, but omits to prove the assertion.

The duration of the sittings he reduces to fifteen or twenty minutes once a day! (He reports five more successful cases in the *Berl. klin. Woch.*, 1889, p. 892.)

Dr. Lahnsen reports a case of tuberculosis cured by Krull's apparatus, and applauds it highly.

Wilson, in a paper read before the American Climatological Association in June, 1889, referring to Weigert's method, says: "Whether or not the tissues of the human lung can preserve their integrity when exposed to air of a temperature ranging from 212° F. (100° C.) to 480° F. (249° C.), sufficiently long to destroy the tubercle bacilli embedded in them, remains to be demonstrated."

Halter, using inhalation of air at 392° F. (200° C.) to 446° F. (230° C.), claims that the temperature of the exhaled air is raised to 110° F. (43° C.), and he refers to the immunity from pulmonary tuberculosis of men employed with lime-baking ovens, which observation has led him to patent a mask for continuous inhalation of hot air. He uses both moist and dry air, and claims decided cures of diphtheria as well as tuberculosis.

With regard to the statement about the lime bakers, the immunity does not appear to extend to stokers, who inhale equally hot air. The very high speed recently developed by the transatlantic steamers has been attended by considerable loss of life among the stokers. Last October one of these men came under my care in the wards of the New York Hospital, suffering from a very acute pulmonary tuberculosis, developed while he was at work in the boiler room of one of the ocean "greyhounds," and the surgeon of the same steamer stated that in his experience such cases were becoming more and more frequent among the stokers. Of course such cases, where the entire body is enveloped in the heated atmosphere, form a very different class from those treated by hot-air inhalation while the body is in an atmosphere of ordinary temperature. I therefore merely cite the above statements to oppose the argument of Halter derived from the lime bakers. I have, furthermore, the authority of an experienced naval surgeon for the statement that, in his experience in the service, he has never noticed or heard that the stokers were less liable to pulmonary diseases than

other men; but, on the contrary, they are more liable to such complaints, not from the fact of their working in an atmosphere of very high temperature, but because, when greatly heated, they are so careless in exposing their bodies on deck.

The claims of Weigert and Kohlschütter that the temperature of the exhaled air rises to 140° F. (60° C.) are not valid, because, if a column of hot air be drawn in at the mouth or nose in ordinary respiration, the last inch or two of the column will be the first to be exhaled with the next expiration, and, having been within the buccal or nasal cavity but a very brief time, it will not be as much cooled as the advance part of the column, which has been drawn deeper in over a larger surface of moist bronchial mucous membrane. The first exhaled air will therefore raise the index of the thermometer and give rise to superficial and erroneous conclusions.

The only observers who thus far have attempted to ascertain whether hot air really *does* enter the lungs without losing its heat, before keeping their patients sucking at a hot-air tube several hours daily for months at a time, are Mosso and Rondelli, of Turin. They separated two simple air chambers by a moist membrane, and succeeded in cooling air from 392° F. (200° C.) to 76° F. (24° C.), by evaporation, in its passage through the membrane from one chamber to the other.

They next allowed a dog to inhale hot air for fifteen minutes at 320° F. (160° C.), while thermometers inserted in the trachea and bronchi, respectively, showed practically no alteration in temperature. The rectal temperature also remained normal.

They used a hygrometer, and found, after inhalation of the hot air, that the exhaled air was as completely saturated as if the temperature of the inhaled air had been 66° F. (18° C.). They also estimated that the expanded hot air brings less oxygen into the lungs in a given time, and that the volume of exhaled air during respiration of hot air was not equal to that exhaled under normal con-

ditions, by nine or ten per cent.—a circumstance which in itself is rather to be deprecated in phthisical subjects.

The results of the experiments of Mosso and Rondelli were entirely unknown to me until my own were completed, and I was pleased to find perfect accordance in our results. In addition, I made numerous other experiments which have not been previously performed, in so far as I am aware. I was interested to undertake these experiments because I could not believe, upon theoretical grounds, in the advantages claimed for inhalations of hot and of cold air, and I desired to convince myself and others by facts derived from experiments.

The theoretical objections to the method of hot-air inhalation appear to me as follows:

I. We are not accustomed to find any important changes in body temperature or in the character of the respiration in those who work in great heat, like founders, bakers, pottery glazers, stokers, and the like, nor in those who are exposed to arctic cold.

II. The widest variation of body temperature admitted by physiologists to occur in those who exchange an arctic for a tropical climate, does not exceed 0.5° to 1° F., and even this is not constant.

III. Very hot air in contact with the mucous membrane of the nose, mouth, pharynx, and larynx, will destroy their epithelium and produce painful swelling and congestion.

IV. If it could reach further, it would cause destruction of the ciliated epithelium of the bronchi, and the delicate cells lining the air sacs would be destroyed and desquamated.

V. It would cause dangerous capillary congestion in the lungs.

VI. The bacilli are many of them buried in large tubercular masses. It is not probable that if hot air could enter the bronchioles or air sacs it could reach more of the bacilli than such as were by chance contained in the sputa. (Weigert does not claim this himself.) To reach

their real foci with sufficient heat to destroy them, the hot air must warm the surrounding blood.

VII. Blood-serum albumin is coagulated at various temperatures, according to the reaction of its solution and the relative amount of salts present. It may coagulate at as low a temperature as 122° F. (50° C.) (Charles), or as high as 163° F. (73° C.). The myosin in the muscles of the trachea and bronchi passes into heat rigor (coagulates) at 131°–140° F. (55°–60° C.) (Charles). The protoplasm of the various cells and blood corpuscles would all be hopelessly injured by the heat.

VIII. The blood itself, unless the pulmonary vessels were greatly congested and the heart action greatly enfeebled, would tend to carry off the heat of the air very rapidly. An enormous surface of blood is exposed in the lungs, which is being constantly replaced with each cardiac pulsation by fresh quantities of blood unheated by the air, and ready to absorb more heat units before they can accumulate sufficiently to damage either bacilli or protoplasm.

IX. The natural process of vaporization of the water from the mucous surface is amply sufficient to absorb all the heat units that can be brought to the bronchi by the superheated air.

X. Only about one-tenth of the air in the lungs is exchanged with each respiration. Suppose, for a moment, that an instantaneous mixture of the air of one inspiration with the residual air of the lungs could occur—which is impossible for several reasons—then one volume of air at, say, 200° F.—nine volumes of air at 100° F.—would reduce the temperature of the mixture to 110° F., and this air would be cooled by evaporation of water long before another inspiration of hot air could raise the temperature still more.

In the face of all these well-known considerations it is difficult to believe that any one could for a moment expect to fill the lungs during life with air hot enough or cold enough to kill bacilli. Koch says that bacilli grow

best at 99° to 101° F. (37° to 38° C.), and cease to multiply at 108° F. (42° C.). Recent researches show that to certainly kill these germs a still higher temperature appears to be necessary.

EXPERIMENTS.—My own experiments were directed to determine:

1. How far can hot and cold air, respectively, be made to convey heat or cold into the air passages?

2. The influence of evaporation upon heat loss.

3. The influence of conduction and radiation upon heat loss.

4. Any changes in body temperature, either local or systemic, produced by the inhalations described.

In the following experiments a very simple apparatus was used for heating the air. It consists of an iron pipe eleven feet long, bent into a close coil three inches in diameter, beneath which are placed several powerful Bunsen burners. Air is forced through one end of the coil in a continuous current from a large treadle reservoir bellows. In traversing the coil the air is heated to 500° F. (260° C.). From the opposite end of the coil the iron pipe is bent out straight for eight inches, and connected with an inhaler made to fit tightly over the head of the animal. The inhaler has a valve which prevents the exhaled air from entering the coil, and this air passes out through a lateral opening in the inhaler. One thermometer is placed in the inhaler close to the animal's nose, and another is placed six inches distant in the tube leading from the coil. In this way the maximum temperature of the air from the coil is recorded, as well as the temperature of the air actually inhaled at the nostrils. The principle is in every way identical with that of the apparatus employed for tubercular cases, but, instead of the animal sucking the hot air through the heater for himself, it is forced through in a rapid, continuous stream by a bellows. In this manner the inhaler and tubes are all kept much hotter, and the temperature at the nose is easily maintained at a continuously high rate. All the thermometers

used for the apparatus or animals were, of course, carefully tested and compared with each other.

The animals were anæsthetized, and thermometers were placed in the lungs or trachea, or both, through small openings which they fitted so closely as to make the holes air-tight. In answer to the possible objection that when the pleura is opened to admit a thermometer the lung might collapse and not admit the hot air, I refer to recent experiments upon the opened pleural cavity made by Drs. O'Dwyer, Phelps, Northrup, and myself, published in the "Researches of the Loomis Laboratory for 1890," which demonstrate that the lung does expand again, as soon as the opening is properly closed, in expiration. As an additional proof, in many cases I inserted a thermometer between the lobes in *each* lung, and so soon as the small openings in the pleuræ were closed, respiration went on normally, and so continued. Hence a thermometer carefully placed between two adjacent pulmonary lobes, or in the lung tissue itself, does not practically interfere in the slightest with the expansion of the lung and the entrance of air in the normal manner.

EXPERIMENT I.—Small dog. Temperature before inhalation: Rectum, 102.5° F. (39° C.); lung, 102.5° F. Air, 120° F. (49° C.) at nose, was inhaled for five minutes. No alteration in temperature of lung or rectum.

EXPERIMENT II.—Dog. Temperature before inhalation: Rectum, 100° F. (37.7° C.); lung, 100.2° F. Hot-air inhalation was started at 150° F. (65° C.), rapidly increased to 200° F., and finally to 220° F. (105° C.). This was the temperature one inch from the nose. The temperature near the coil was 200° F. higher. Inhalation was maintained for one and a half hours. During this time the temperature in the lung fell 1.2° F., and in the rectum 2° F., while the respiration fell from 26 to 14. The animal took the chloroform very ill, and a great deal was administered, so that a slight chill ensued, with the fall of temperature observed in the lung and rectum. Clearly there was no entrance of hot air to the lung in this case. The animal was subsequently allowed up, and it appeared bright and well.

EXPERIMENT III.—Small dog. Temperature of lung before inhalation, 104° F. Inhaled air at 350° F. (177° C.) at nose for fifteen minutes, at the end of which time the temperature of the lung had risen only 1.5° F. On being allowed to get up, the dog showed panting respiration and a very red tongue, with congested lips. It should be remembered that the heat of the blood which is ordinarily lost in warming the inspired air, when that air is below the blood temperature, is no longer so lost when the inspired air is very hot. This may account for the rise of a degree or two in the temperature of the blood, which is due to accumulated heat rather than to heat brought in by the air. It is estimated that 2.6 parts per 100 of the heat produced by the body is normally lost in warming the inspired air (McKendrick), chiefly in the nose and trachea.

EXPERIMENT IV.—Bitch, with thermometer placed in right lung. Before inhalation, temperature in vagina 102.2° F. (39° C.); in right lung, 103.6° F. (39.6° C.). The inhaler was worn one and a half hours, and the air was started at 180° F. (82° C.) and rapidly increased to 290° F. This was the temperature close to the nose and mouth, corresponding to the position in which the writers above quoted claim to have taken the temperature of "expired air," except that my valve and thermometer were a little nearer the mouth. During the first hour of inhalation of air at considerably above 200° F. (93° C.), the vaginal and lung temperatures were frequently noted, and they remained absolutely unchanged from the record prior to the inhalation. Surely, if air at 200° F. (93° C.) were to raise the temperature of the lungs at all, it would do so within an hour's time. During the next half-hour, as the temperature of the inhaled air was raised to 290° F. (143° C.) at the nose, the animal became very uncomfortable and restless, and there were evident signs of local irritation from the heat, such as profuse salivation, red and swollen tongue and lips, and congested conjunctivæ. Under these conditions a rise of body temperature occurred amounting to 1.8° F. (1° C.), so that the vaginal temperature at the end of one and a half hours registered 104° F., and the lung 105.2° F. (40.5° C.). This rise of 1.8° F. or more in the body temperature often occurs when a dog is restrained in an irksome position for some time and subjected to almost any kind of operation. I have frequently seen the temperature of animals rise 2° F.

while they were struggling to resist an anæsthetic. It does not seem to me fair to refer the slight rise in the final half-hour of this experiment to any effect of the heat, other than the local discomfort and irritation produced near the inhaler.

EXPERIMENT V.—Large bitch. Thermometer in trachea close to larynx, before inhalation, 102.2° F. (39° C.); vaginal temperature, 105° F. (40.5° C.). Air six inches from nose started at 240° F. (115° C.), and rapidly heated to 440° F. (227° C.). At the nose the temperature was 200° to 315° F. (157° C.). Inhalation maintained one and a quarter hours, during which time the temperature in the trachea rose only 4° F., *i.e.*, to 106.2° F. (41.2° C.). The tracheal temperature showed interesting fluctuations of 0.4° F. between deep inspiration and expiration (measured by a thermometer without the usual detached index). Air which is cooled over 200° F. in entering the trachea, traversing a distance of only six or eight inches from the nose, is not likely to convey much heat through a greater distance beyond, into the alveoli!

EXPERIMENT VI.—Large cat. Temperature before inhalation: In lung, 100.4° F.; in rectum, 99° F. Respiration, 40. Inhalation was begun at 200° F. at cat's nose, and rapidly raised to 300° F. (149° C.) at the nose, and continued for one hour. Six inches nearer the apparatus the thermometer recorded 425° F. (218° C.). For fifteen minutes there was no rise in the temperature of the lung or rectum. The cat then began to exhibit great local irritation about the face; the eyes were suffused, the lips and tongue very red and swollen, and the respiration in half an hour became very shallow and ran up to over 200. At the end of an hour's inhalation of hot air between 260° and 300° F. (149° C.) at the nose, the lung and rectal temperatures both rose 6.8° F. The inhalation was then suspended, the cat being very feeble, and in one hour more respiration and temperature were normal and the cat walked naturally about.

EXPERIMENT VII.—Dog. Temperature before inhalation: Lung, 102.6° F.; rectum, 102.2° F. (39° C.). Inhalation started at 200° F. (93° C.) at nose, and was rapidly increased to 280° F. (138° C.), and so continued. In ten minutes there was no rise of temperature. At the end of three-quarters of an hour the lung and rectal temperatures had both slowly risen 2.4° F., and during the

remaining fifteen minutes of inhalation there was no further rise. The animal exhibited great irritation about the nose and mouth.

EXPERIMENT VIII.—Large cat. Inhaled air from 250° F. to 270° F. (132° C.) at the nose (500° F. in the tube of the apparatus). Inhalation maintained for twenty minutes, during which time the temperature of the lung rose from 100° F. to 102° F., and the temperature of the rectum from 101° F. to 103° F. Respiration became panting, and very shallow and rapid, finally reaching 250 per minute. The tongue and lips became intensely red and swollen; there were salivation and thirst, with every indication of great discomfort about the head, which was completely enclosed in the inhaler. I have frequently seen a similar rise of temperature in cats, irritated in other ways, under anæsthesia. A temperature of 270° F. (132° C.) at the mouth and nose is hot enough to irritate any animal into panting respiration, with great disturbance of the circulation and heat-regulating mechanism. The 2° F. of elevation of the lung temperature in this case is a very trifling one, were it really due to the heat of air inhaled at 270° F. (132° C.), and because a similar slight rise may be produced in other ways, merely from irritation of various kinds. I cannot believe that in this instance the heat of the air was conveyed to the alveoli direct.

EXPERIMENT IX.—Dog. Two thermometers placed in the trachea, one at the larynx, the other two inches below. These thermometers varied from 103° to 103.4° F. (39.5° C.), fluctuating 0.4° F. with each respiration before the inhalation. Hot air was then inhaled through the nose, where the temperature was 240° F. at the start, rapidly elevated to 300° F. (149° C.), with only 1.2° F. rise in the tracheal temperature. The animal then died from heart failure under chloroform. Tracheotomy was at once performed, and the hot-air tube was inserted at the first ring of the trachea. The second thermometer, two inches below, was left in the trachea. Through the tracheotomy tube artificial respiration at 300° F. (149° C.) was maintained for ten minutes. The chest was opened, a small hole was cut in the lung, and a long-stem thermometer was placed directly in the lung substance. The chest was closed, and the thermometer index was read from the outside. During the ten minutes of forced

respiration the temperature of the lung rose from 103.2° F. to 106° F. (41° C.), less than 3° F.! The lungs were then excised, with the thermometers and tracheotomy tube *in situ*, and artificial respiration was continued outside of the body for twenty minutes more. The temperature of the air on entering the trachea was between 320° F. and 355° F. (180° C.). The tracheal temperature two inches below could not be made to rise above 140° F. (60° C.), and the temperature of the lung tissue gradually fell to 103° F.

These experiments show: I. That when air at a temperature of 300° F. + is forcibly driven into the lungs of a recently killed dog, the moisture of the lungs, notwithstanding the stoppage of the normal circulation, is still sufficient to cool the inhaled air to nearly the temperature of the body. II. The cooled air of exhalation prevents the temperature in the trachea from rising above 140° F., although the hot air of inhalation exceeds 300° F. During a part of the experiment, I endeavored to fill the lungs much more rapidly and fully than in normal breathing; but even these efforts failed to raise the lung temperature beyond 2.8° F. above the normal.

In view of the statement that a temperature above 102° F. retards the growth of tubercle bacilli, it is of interest to note that the normal temperature of rabbits is 102.5° to 103° F.; that of guinea-pigs, 103° F.; of monkeys, 102° to 102.5° F.; of pigeons and fowls, 108.5° to 110.5° F.; whereas it is well known that all of these animals are readily affected by pulmonary and other forms of tuberculosis.

Experiment X.—The laws of the relations of temperature to evaporation of fluids are too familiar for repetition here, but the following simple experiment is a striking illustration of the rapidity with which the heat of the air may be radiated from a non-isolated conducting tube, and of the rapidity with which it becomes latent in vaporizing water.

Through one end of a piece of rubber tubing, 3½ feet long, of ⅝ inch bore, and 1½ inch thickness, a strong, continuous current of air was forced as rapidly as a large

reservoir bellows could be operated. The temperature of the air at the tube entrance was recorded, at a distance of fourteen inches it was again recorded, and at the exit, three and one-half feet distant, a third thermometer was placed. The air was forced through for some minutes, when the temperature read as follows: Entrance, 260° F.; 14 inches from entrance, 205° F.; 3½ feet from entrance, 130° F. The tube was then moistened on the inner surface by pouring water through it. All the water was removed, except the little which wet the inner wall of the tube. Hot air was again forced through for some minutes at 280° F. (138° C.), when the exit thermometer recorded only 90° F. (32° C.). Therefore, merely moistening the tube caused a fall of 40° F. in the exit temperature.

EXPERIMENT XI., showing the effect upon the hot air of vaporization of water.—Large mongrel bitch weighing eighty pounds. Normal temperature; Rectum, 103.5° F.; vagina, 103.5° F. The abdomen was opened, and the large intestine was cut in two. A half-inch glass tube was inserted in the distal end, and a stream of water was forced through the intestine toward the anus, to wash out the rectum. The intestine being clean and empty, a large glass nozzle was fitted air-tight into the rectum, and through it a stream of hot air was blown, which traversed the large intestine and passed out at the abdominal opening, where a thermometer recorded the exit temperature. The temperature of the injected air at the rectum was started at 200° F.; in eight minutes it rose to 300° F., at the end of half an hour to 340° F., while the entrance temperature rose from 200° F. to 300° F.; the exit temperature rose from 90° F. to 98° F. only. Later, as the air current was greatly quickened and the intestine became a little drier, the exit temperature rose to 120° F., but I was unable to get it above this limit, though the hot-air current was continued twenty minutes longer (fifty minutes in all). To estimate roughly the relative quantity of moisture in the dry, hot entrance air and the same air after traversing the moist mucous surface of the intestine, I used a hygrometer. The instrument was not accurate, as the scale was somewhat arbitrary, but it served to show the relative moisture. Upon this hygrometer scale of 100° the air of the room stood at forty-seven per cent. My own deeply exhaled air indicated seventy-eight per cent., the hot, dry air at the dog's rectum ten per cent., and on emerging from the intestine

above, sixty-two per cent. During the entire experiment the mouth temperature remained unaltered at 102° F. The abdominal incision was then closed, and the animal, after recovering from the chloroform, ran actively about the laboratory. An hour later she was killed. The injected hot air had been so thoroughly cooled that I expected to see that several feet of intestine had been traversed, but great was my surprise to find that the distance from the intestinal incision to the rectum was only five inches. In other words, the moisture of five inches of intestinal wall by evaporation cooled the air from 340° F. down to 130° F., and maintained this reduction through fifty minutes, although the air current was as rapid as a large bellows could make it. Curiously enough, the dog, constantly anæsthetized, was far more distressed during the experiment than were any of the other dogs after inhaling hot air for even longer periods. At one time the respirations rose to 98 and were very labored, and I was obliged to discontinue the injection to allow the animal to recover, which she did very promptly.

Cold Air in Hæmoptysis, Pneumonia, and Tuberculosis.—Dr. P. de Tullio, of Naples, has recently suggested the employment of cold-air inhalations for control of pulmonary hæmorrhage. He forced air through a coil of tubing surrounded by ice and salt, and cooled the air to 0° C. He claims to have checked hæmoptysis in three severe cases, after failure with other means, such as drugs, ice to the chest, etc. In the *Medical Recorder* for 1889, Dr. G. Lipari, of Palermo, in discussing the etiology of pneumonia, thus explains the influence of cold air in occasioning that disease. He says that cold atmospheric air, when inhaled, paralyzes the ciliated epithelium of the bronchi, and causes their mucous membrane to swell, and this favors the descent of the pneumococci into the alveoli.

So much for theory. The facts concerning the supposed inhalation of cold air are no less striking than those above cited in regard to inhalation of hot air. I took the same coil used for the hot-air experiments and embedded it in a mixture of snow and salt; the bellows was operated outside of the window, in an atmosphere of 27° F. In

this way the air after passing through the coil registered less than 40° F. at the animal's nose.

Koch says that a temperature of 85° F. (29° C.) must be produced before tubercular bacilli are checked in their growth.

EXPERIMENT XII.—Large dog, chloroformed. Temperature before inhalation: In trachea, 99.5° F.; in lung, 99.6° F.; in vagina, 101° F. Inhalation continued one and a quarter hours. Temperature in coil, 30° to 27° F. Temperature at nose, at commencement, 65° F., rapidly reduced (as inhaler was cooled by air from coil) to 40°, and finally to 37° F.

Temperature in the trachea fluctuated 4° with each respiration. During crying, under partial anæsthesia, it would fall with very deep inspiration 1° to 2° below the average record in the trachea at the time. The tracheal temperature gradually fell, and at the end of one and a quarter hours the total fall was 10.4° F., whereas after three-quarters of an hour interval the temperature of the lung remained unaltered, and in the remaining half-hour, when the room and the animal's body grew cold, it fell only 2° F., and the vaginal temperature fell 3° F.

The animal recovered promptly his condition when allowed to get up, and seemed uncommonly lively. A fall of only 2° F. in the temperature of the lung, after one and a quarter hours of inhalation of air below 40° F., will never be sufficient to control pulmonary hæmorrhage through vaso-motor stimulation. No one would think of controlling capillary oozing on the surface of the body by reducing the local temperature only 2° F.! Even should the hæmorrhage come from the vessels of the upper trachea, the fall of only 10° F. there would not control it. It often requires prolonged direct application of ice to produce sufficient vaso-motor stimulus to control hæmorrhage, and even that has frequently failed in post-partum hæmorrhage.

EXPERIMENT XIII.—Cat, chloroformed. Temperature before inhalation: Lung, 99° F.; rectum, 99° F. Inhalation of air at 40° F. (at nose) was continued for one hour, when the temperature of the trachea was found to have fallen 5.2° F. The temperature of the lung fell only 3° F., and that of the rectum 4° F. I have often seen the temperature of a cat fall considerably more during operation

under prolonged anæsthesia. There was 1° F. of fluctuation between inspiration (fall) and expiration (rise) in the trachea.

It is very difficult to conduct air from any cooling apparatus to the mouth without its gaining a few degrees in temperature if the room is warm. I have found it so even when external air was used at 27° F. and forced through an iron coil, eleven feet long, embedded in snow and salt. It would be doubly so in summer, when the warm atmospheric air must first be cooled in the coil. It is possible to cool air in this manner to nearly the freezing point, but it would be hardly practicable to do so for cases of sudden pulmonary hæmorrhage. Moreover, the cool air, as it enters the mouth or nose, meets a surface constantly warmed by the air of exhalation and by the local blood stream. If, every time that we stepped from a furnace-heated room on a winter morning into an atmosphere below the freezing point, the temperature of our lungs fell 10°, or even 2°, we should have to be constructed on the principle of batrachians to survive it.

In a discussion before this Academy on November 26th, 1889, Drs. Beverley Robinson and J. M. W. Kitchen called attention to the clinical fact that patients who have been using warm inhalations are particularly liable to contract colds on going too suddenly into a cooler atmosphere. This is doubtless true, but I think that it must be attributed to the influence of subsequent atmospheric changes upon the upper air passages, and not to any effect upon the bronchi or lungs.

In performing all the experiments reported, I have received very valuable assistance from Dr. T. D. Coleman, and from Messrs. W. Coleman, Coudert, and Holden, students at the University Medical College.

Conclusions.—The following conditions combine to secure a uniform mean temperature of the air in the lungs, and prevent the entrance of superheated air, as such: The great vascularity of the pulmonary tissue; the extensive surface of the air sacs and blood vessels; the slow

diffusion of the tidal with the residual air; the uniform temperature of the blood; the remarkable heat-regulating mechanism of the entire body, which constantly tends to prevent local increase of temperature through the agency of a very rapid blood stream; the low specific heat of water, and of the blood which is so largely composed of water; the large quantity of blood in constant circulation, approximately fourteen pounds in man; the enormous number of heat units rendered latent by vaporization of water, and the ease with which water can pass from the blood plasma to the surface of mucous membranes, in considerable quantity; the conduction of heat by the body tissues.

Conclusions derived from the preceding experiments upon cats and dogs:

1. Continuous inhalation of air heated from 200° to over 300° F. at the nose does not raise the temperature of the lungs at all in some cases, even when inhaled for an hour or more.

2. In other cases, if continued for an hour and more, there may be a slight rise of from 2° to 4° F., due to various causes other than the entrance of hot air into the alveoli.

3. The temperature of the trachea under corresponding conditions rises only 4° to 7°. Cold air does not affect the temperature of the trachea or lungs any more than hot air, and is therefore equally useless as an inhalation for any clinical purposes whatever.

I have received, too late for review in this article, a very valuable paper upon the theoretical objections to the use of the hot-air inhalation treatment. It is by Dr. W. L. Cary, physician to the Brooklyn Throat Hospital, and it is altogether the most able and scientific refutation of the hot-air-inhalation theory, based upon purely theoretical grounds, which has yet appeared.

There are doubtless many physiological experiments in regard to which one must not reason too closely from the lower animals to man, but it seems to me that the funda-

mental conclusions derived from the experiments herein reported are of a type which must necessarily apply to man as truly as to the lower animals, because they are based upon sound laws of physics, and they afford one more illustration of the great value of vivisections, conducted with the object of preventing the employment of methods powerless for the relief of human diseases, the ignorant use of which can only bring great discomfort to the patient.

BIBLIOGRAPHY.

Mosso and Rondelli: Einathmung auf 200° erhitzter Luft, Deutsch. med. Woch., No. 27, 1889, p. 535.

Weigert, L.: Die Heissluft Behandlung der Lungentuberculose. Berlin, 1889. Monograph.

Kohlschütter: Die Heissluft Behandlung der Lungentuberculose. Berlin, 1889. Monograph.

Halter, Louis: Berl. klin. Woch., September 3d, 10th, 17th, 1888.

Krull, Ed.: Berl. klin. Woch., September 24th, 1888.

Krull, Ed.: Berl. klin. Woch., 1889, pp. 607, 813, 892.

McKendrick's Physiology, ii., 440, 443.

Weigert, L.: New York Medical Record, December 15th, 1888, p. 693.

Schmid: Münchener med. Woch., 1889, No. 24.

Lahnsen: Berl. klin. Woch., 3. Juni, 1889, p. 501.

Thompson, W. Gilman: Researches of Loomis Laboratory, No. 1, 1890, p. 63.

Charles: Physiological Chemistry, pp. 107, 109.

Cary, W. L.: Criticism of Weigert Hot-air Treatment, Brooklyn Medical Journal, April, 1890, p. 193.

Halter: Internat. klin. Rundschau, December 2d, 9th, 1888; No. 10, 1889.

Lazarus, J.: Deutsch. med. Woch., 1889, p. 153.

Maryland Medical Journal, 1889, xxi., 261.

Journal of the American Medical Association, 1888, June 25th.

Shurley, E. L.: New York Medical Record, xxxvi., 1889, p. 25.

Sears: Boston Medical and Surgical Journal, cxxi., 1889, p. 33.

Stern: New York Medical Record, April 27th, 1889, p. 469.

Jacobi, A.: New York Medical Record, xxxvi., 1889, p. 53.

Koch: Mittheil. aus dem kais'l. Gesundheitsamt, 1884, p. 58.

De Tullio, P.: Journal of the Respiratory Organs, p. 13, 1890.

Lipari: Lancet, i., 1890, p. 418.

Robinson and Kitchen: Report of Meeting at New York Academy of Medicine, November 26th, 1889, New York Medical Record, December, 1889.

Bowie: Lancet, ii., 1889, p. 540.

Wilson: Medical News, Philadelphia, lv., 1888, p. 234.

Trudeau: Medical News, Philadelphia, lx., 1889, p. 337.

Helmholtz: On Vaporization of Water.

A STUDY OF CEREBRAL PALSIES OF EARLY LIFE, BASED UPON AN ANALYSIS OF ONE HUNDRED AND FORTY CASES.*

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FEW diseases are better known or more thoroughly understood than infantile spinal paralysis. Its clinical symptoms and its pathology have been definitely determined, so that poliomyelitis anterior acuta scarcely needs further study. The very opposite is true of infantile *cerebral* palsy. While there is but a single form of disease included under the term infantile spinal paralysis, we are forced to admit that there are several different forms of cerebral palsy. The attempt to fix upon certain cases of cerebral paralysis in children, and to label them infantile cerebral palsy as a direct analogy of the better known spinal disease, has led to great confusion, although we shall see that there is some justice in drawing this analogy. Nor is it entirely correct to speak of cerebral *spastic* palsies as distinguished from *atrophic* paralysis, for we shall report at least two cases in which the element of spasticity was entirely wanting, although the cases were undoubtedly of cerebral origin, the proof of which was furnished by the post-mortem examination in one of these two cases.

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* Read at the stated meeting of The New York Academy of Medicine, April 3d, 1890.

The large majority of the cases with which we are here concerned represent spastic forms of paralysis, and, as regards the distribution of the palsy, may very properly be divided into cases of spastic hemiplegia, of double spastic hemiplegia, or diplegia, and of spastic paraplegia. Since the clinical subdivisions are so easily made, it would seem to be a curious fact that these cases have been so poorly understood and so little studied. There were many reasons for this: first and foremost, the great difficulties in obtaining autopsies, the majority of these cases either living on to a very advanced age, or else dying in almshouses where no interest was taken in them; then, again, the condition was so frequently associated with idiocy; or the individuals were regarded as hopeless cripples that did not possess sufficient interest to repay careful study; and, lastly, the term infantile cerebral palsy proved to be a stumbling-block. A number of cases and some few autopsies were reported, disclosing a variety of lesions; there seemed to be little hope of bringing order out of chaos. The truth of the matter, that we had a number of different forms of disease and a variety of pathological processes to consider, was a long time forthcoming. We hope to show by this paper that much of the confusion that has surrounded this subject will be removed if we consider that a variety of morbid symptoms may give rise to any of the three forms of paralysis, and that the character of the paralysis will depend upon the site and extent of the morbid lesion. But for the difference in the areas of the brain affected and the degree of irritation or destruction of brain substance, the symptoms in all these cases would be very much the same.

The subject which we present has a live interest at this present time, and yet it is nearly fifty years ago since the first work in this field was done. In 1842 Prof. Henoch wrote his inaugural dissertation, "De Atrophia Cerebri," and gave an excellent account of infantile cerebral paralysis.[1] Heine[2] referred to these cases in a monograph on

NOTE.—The small figures refer to the references at the end of the paper.

spinal paralysis of children, published in 1860. Little[3] was well acquainted with them. In 1868 Benedikt,[4] the neurologist, described them. The French schools soon took up the subject, and Cotard,[5] Wuillaumier,[6] Bourneville[7] wrote important papers on spasmodic infantile paralysis, some of them referring to the anatomical features of these diseases. As recently as 1883, several English authors (among them Hadden[8] and Ross[9]) published the clinical details of a small number of cases and the accounts of a few autopsies.

Two publications have stirred up the recent discussion on this question. The first was Kundrat's[10] monograph on porencephalus, in which this one morbid state was carefully studied. The second was Strümpell's[11] paper in 1884, in which he suggested that infantile cerebral hemiplegia was due to a polioencephalitis acuta. This single statement, which was decidedly original but had no post-mortem proof, has raised a great hue and cry which have not yet subsided. Whatever other good it may have accomplished, Strümpell's theory has at least imbued an old subject with new life. His article was quickly followed by a number of valuable contributions, among which those of Ranke,[12] Bernhardt,[13] Wallenberg,[14] Kast,[15] Jendrássik and Marie,[16] Gowers[17] and Hoven[18] are by far the most important of the European contributions. In America able articles have been written by Dr. Sarah McNutt,[19] Drs. Sinkler,[20] J. Lewis Smith,[21] Knapp,[22] Lovett,[23] and Gibney.[24] Chief and foremost of all is an exhaustive monograph by Prof. Osler,[25] in which one hundred and fifty-one new cases were analyzed with a skill which characterizes all of Prof. Osler's work.

There would seem to be some need of an apology after this for the study of another series of cases. Our first reason for doing this is, that the work preparatory to this paper was begun several years ago, before the publication of Prof. Osler's monograph; secondly, we had some views of our own to advance; and, lastly, the unusual number of cases at our disposal called for special elaboration.

The cases here reported upon were, with very few ex-

ceptions, examined by one of the authors of this paper. We are greatly indebted to Dr. Gibney and Dr. Townsend for referring the greater number of these cases to us from the Hospital for the Ruptured and Crippled. Dr. Gray was kind enough to allow us to use the notes on cases seen in his department. Ten of the cases were seen in private practice.

The physician is no longer content, or at least should not be, to make the diagnosis of apoplexy, of hemiplegia, or of paraplegia, in the adult. It is his aim to determine whether the special form of paralysis be due to hæmorrhage, thrombosis, embolism, tumor, abscess, or what not. In short, he studies the symptoms of each case with special reference to pathology of the disease. And so with infantile palsies: it is not enough to recognize spastic hemiplegia, diplegia, or paraplegia, but the attempt should be made to determine the special morbid condition underlying each form. The large number of cases made it imperative upon us to make a distinct effort in this direction.

In view of the small number of autopsies recorded in literature, this effort may seem hazardous; but a single autopsy in a well-observed case is a guide in the study of dozens of others; and with the advances made in a knowledge of cerebral lesions, the neurologist is fortunate in being able to reason with a great degree of certainty from clinical symptoms to the morbid lesion which they indicate.

A thorough knowledge of the clinical symptoms is, however, the starting point of this study. In order not to weary you with the details of these cases, we give you the conclusions to be drawn from our entire series with reference to each point at issue. And we shall give but a few histories *in extenso*, that you may recognize the chief types of which we treat. Before proceeding to these cases, we submit a table showing that of the one hundred and forty cases* there were eighty-seven males and fifty-three

* Since the above was written we have seen at least fifteen additional cases, but we have concluded not to alter the tables in the body of this paper.

females; there were one hundred and five hemiplegias, twenty-four diplegias, and eleven paraplegias.

TABLE I.

Showing the sex and form of paralysis in 140 *cases of infantile cerebral palsy.*

Form of Paralysis.	Males.	Females.	Total.
Right Hemiplegia	37	15	52 } 105
Left Hemiplegia	26	27	53 }
Diplegia	15	9	24
Paraplegia	9	2	11
Total	87	53	140

HEMIPLEGIAS.

CASE I.—(No. 48.) A. F., æt. four and a half years; male; first child; difficult labor and instrumental delivery. From very first day right hemiplegia. Slight athetosis and associated movements. Contracture at elbow, formerly pes equino-varus, improved by operation. All reflexes of right side lively. Mental condition fair.

CASE II.—(No. 58.) L. B., female, æt. nine years. Congenital left hemiplegia. Labor normal; fourteenth child; movements of child ceased a few days before birth; expected to be still-born; weighed four pounds at birth. From age of two years epileptic attacks every two to four weeks. Fine associated movements of both sides. Arm, leg, and face of left side involved. Very great retardation of growth of arm and leg; some contractures of flexors of arm and wrist; imbecile; microcephalic.

CASE III.—(No. 44.) L. E., female, æt. eight years; acquired hemiplegia; onset at age of five years; convulsions for nine hours and coma; could not move, talk, or walk for three months. Face and leg have recovered considerably; very great retardation of growth of arm. Marked associated movements. Contracture of right arm; reflexes of right side exaggerated.

CASE IV.—(No. 138.) J. K., male, æt. seventeen years. Right hemiplegia at eight years of age following typhomalarial fever; was delirious and unconscious during nineteen days; no convulsions. After recovering from coma, right arm, face, and leg were found paralyzed. Complete

aphasia and entire loss of memory of everything occurring before typhoid. Had to be re-educated. Athetoid and associated movements. Reflexes exaggerated right side. Enormous contracture of flexors of right hand and fingers and great retardation of growth of right upper extremity. Right leg somewhat smaller than left; right talipes valgus; asymmetry of face. Electrical reactions and sensation entirely normal. Has recovered speech fully and is bright, but several years behind others in education.

DIPLEGIAS.

CASE V.—(No. 141.)* J. O., female, æt. sixteen. Congenital diplegia; mother kicked in abdomen by horse two months before birth of child and made unconscious thereby. Three other children, all healthy. Tedious labor, no instruments used, no fits or convulsions. Did not attempt to creep or walk; teeth at usual age. Patient has menstruated since tenth year, and was weak in back, arms, and legs from earliest childhood. Extreme spastic contracture of adductors and flexors of thighs; double talipes varus, equinus on right side. Left arm worse than right. Athetoid movements of left hand. Has frog walk. Intelligence good.

CASE VI.—(No. 31.) M. L., male, æt. three years. Congenital diplegia. Asphyxiated during labor. Mother had pneumonia, and died five days post partum. Rigidity of arms, legs, and back. Hands did not unclinch for two years. Frequent convulsive seizures alternately of right and left side, including face. Cannot talk, walk, or stand. Feeble-minded; cross-legged position, and all reflexes exaggerated.

PARAPLEGIA.

CASE VII.—(No. 50.)† C. F., male, æt. one year. Congenital paraplegia. First child; labor hard, and dry for forty-eight hours. Asphyxiated. From first day up to age of six and a half months, child had a rapid succession of tonic and clonic spasms affecting all the muscles of the

* This case, although under observation for a long time, was omitted from our list by mere accident. The history is sufficiently characteristic to deserve special mention.

† While this article was passing through the press, this child died. An autopsy was obtained by Dr. L. E. Holt. A careful study of the pathological findings will be made and published in due time. But one other autopsy on infantile paraplegia has hitherto been published.

body, causing rigidity of all extremities, opisthotonos with extreme arching of back, enormous exaggeration of all reflexes, ankle and quadriceps clonus on slightest excitation. Convergent strabismus; crying continually. Mental condition probably imbecile. Epileptic spasms controlled slightly by bromide treatment.

CHIEFLY MONOPLEGIA.

CASE VIII.—(No. 47.) H. K., male, æt. twenty-two months. Acquired right hemiplegia; onset at six months following convulsions during pertussis and pneumonia. Trace of weakness in right arm; distinct spastic paralysis of right leg; knee-jerk exaggerated; feeble-minded. This case made at first the impression of a monoplegia.

FLACCID PARALYSIS.

CASE IX.—(No. 61.) M., female, æt. two years at time of death. First-born of healthy parents. During the fifth month of pregnancy mother was thrown from a carriage without sustaining any serious injuries. The child born at full term, apparently normal in all respects. At age of two to three months a general listlessness and nystagmus were observed. During its entire life child was unable voluntarily to move any muscle of its body. All muscles extremely flaccid, but all reacted perfectly to electrical currents. There was not the first symptom of any mental awakening. During the first year of its life child noticed light, but later on absolute blindness set in. There was a developmental defect of the optic nerves, which was reported upon by Dr. Knapp, a similar condition of the nerves having been observed in only two other cases. Hearing seemed to be acute. There was unusual hyperexcitability of auditory and tactile impressions. The child never had convulsions, not even during dentition; no rigidities. All reflexes lively. Speech was, of course, entirely wanting. The child died of pneumonia following bronchitis. The autopsy will be referred to later on. This case was made the subject of a special paper by one of us[26] (S.), and entitled "On Arrested Cerebral Development, with Special Reference to Cortical Pathology." This paper was a study of some of the cortical changes giving rise to idiocy, but we have since come to learn that the report of the case and the autopsy have a wider significance than was attributed to them at the time.

CASE X.—(No. 80.) H. M., male, æt. eleven and three-quarter years. The second of three children; an uncle said to have been similarly affected. Asphyxiated at birth; instrumental delivery. Began to teethe late; teeth have rotted away. Made imperfect attempts to walk at fourteen months; crept around on his buttocks; both feet turned inward, right more than left; learned to talk, but mind has always been very feeble. Has had frequent epileptic attacks, grand and petit mal. Both upper extremities excessively weak, but no rigidities. Lower extremities poorly developed. All muscles respond to faradic current, but some of them so feebly that very strong currents are needed. Knee-jerks weak but present; boy's father is a teacher, who has done much with his defective mind.

From the records herewith presented to you, you will infer that, much as the cases differ from one another, they also have much in common; they yield a distinct composite portrait. The child is either born with, or in its early life develops, some form of paralysis—a hemiplegia, a diplegia, or a paraplegia. In the congenital cases there has been some disturbance during pregnancy, or labor has been tedious and difficult, or definite cause cannot be given. In the acquired cases we have seen that the onset of paralysis may occur after acute infectious diseases, during convulsions, or from causes that cannot be fathomed. In the majority of cases there are marked spasticity and extreme contractures; in two cases there is a flaccid form of paralysis; in the last case reported, the knee-jerks and other reflexes were weak; in all other cases the reflexes were exaggerated, at least on the side or sides paralyzed. Some show peculiar associated athetoid or other post-hemiplegic movements; in all there was more or less retardation of growth, and all stages of mental impairment were found, from weak-mindedness to complete idiocy; a few, however, are of good mental development. No changes in sensibility were observed, and the electrical reactions were never markedly altered.

These cases and some of the symptoms they exhibit are referred to by the poet, historian, and the painter. The

Duchess of Gloucester, according to Sir Thomas More, had much ado in her travail. Her son, Richard III., came into this world feet foremost. Shakespeare makes Richard say of himself:

> "Deformed, unfinished, sent before my time
> Into this breathing world, scarce half made up,
> And that so *lamely* and unfashionable
> That dogs bark at me as I *halt* by them."

In Raphael's Transfiguration, the demoniac boy has characteristic athetoid position of one hand.

It is now in order to see whether our cases shed any new light on the understanding of these palsies, and what relation the facts hold to the commonly received classification into three distinct subdivisions.

TABLE II.

Showing the age at onset.

Age at onset.	Hemiplegia.	Diplegia.	Paraplegia.	Total.
Congenital	22	20	7	49
During 1st year	27	1	2	30
" 2d "	17	1	..	18
" 3d "	16	2	1	19
" 4th "	4	..	..	4
" 5th "	4	..	..	4
" 6th "	2	..	..	2
" 7th "	1	..	..	1
" 8th "	5	..	..	5
" 9th "	..	..	..	..
" 10th "	..	..	..	..
Under fifteen years	4	..	..	4
Unascertained	3	..	1	4
Total	105	24	11	140

From the above table we learn that of one hundred and five cases of hemiplegia, twenty-two are congenital; of twenty-four cases of diplegia, twenty are congenital, and so are seven or possibly eight of the eleven cases of paraplegia. Diplegias and paraplegias are more likely to be of congenital origin, hemiplegias are more apt to be acquired in the first three or four years after birth; but it is well worth noting that there are twenty-two congeni-

tal cases of hemiplegia (over twenty per cent of all cases of hemiplegia); some of the cases noted as occurring in the first year may be congenital, and this would help to swell the percentage of congenital hemiplegias. It will not do, therefore, to make the broad distinction so frequently made on the basis of the acquired or congenital character of these palsies. Our table is in happy agreement with the one given by Osler as regards hemiplegias (fifteen of one hundred and twenty were congenital). Sixty of our hemiplegic cases were developed before the close of the third year of life; then there is a distinct falling off up to the age of ten; between ten and fifteen we have a few more; in three cases the age could not be ascertained. Will you also note that some of the acquired cases of paraplegia or diplegia have occurred after the age of three years, and one or the other of these cases may have been congenital.

TABLE III.

Showing age at examination.

Age.	Hemiplegia.	Diplegia.	Paraplegia.	Total.
Under four years	40	16	8	64
Between four and ten years	26	6	2	34
" ten and twenty years	14	2	1	17
" twenty and thirty years	18	..	..	18
Under forty years	7	..	..	7
Total	105	24	11	140

This table records the ages at examination, from which it is apparent that diplegia and paraplegia are comparatively short-lived, while hemiplegias often attain a very considerable age. Statistics of the exact ages at death would be more accurate; but inasmuch as our material includes cases from every kind of institution, even from pauper asylums, the inferences to be made are tolerably correct.

TABLE IV.

Showing causes given in ninety-one cases of acquired cerebral palsy.

Causes given.	Hemiplegia.	Diplegia.	Paraplegia.	Total.
Convulsions	20	1	..	21
Pneumonia	6	..	..	6
Trauma to head	6	..	..	6
Pertussis	4	..	..	4
Measles	2	2	..	4
Scarlatina	3	..	..	3
Onset with fever	2	..	1	3
Hereditary syphilis	2	..	..	2
Cerebro-spinal meningitis	2	1	..	3
Onset with fever and convulsions only	2	..	..	2
Fright	2	..	..	2
Hydrocephalus	..	..	2	2
Vaccinia	1	..	..	1
Typho-malarial fever	1	..	..	1
Small-pox	1	..	..	1
Tonsillitis	1	..	..	1
Epileptic seizure	1	..	..	1
Gastro-enteritis	1	..	..	1
Unascertained	26	..	1	27
Total	83	4	4	91

TABLE V.

Showing ascertainable causes in forty-nine cases of congenital cerebral palsy.

Causes.	Hemiplegia.	Diplegia.	Paraplegia.	Total.
Instrumental delivery, tedious labor	4	1	1	6
Ante-partum trauma to mother	2	3	..	5
Premature birth	1	2	1	4
Asphyxia at birth	..	3	..	3
Asphyxia in twin birth	..	1	1	2
Tedious labor, breech presentation	2	..	..	2
Primipara, dry birth, tedious labor	..	..	1	1
Seven months' child, dry birth (forty-eight hours)	..	1	..	1
Primipara (æt. forty-five), tedious labor	..	1	..	1
Maternal fright (ante-partum)	1	..	..	1
Uræmia of mother	1	..	..	1
Pneumonia of mother (died five days post-partum), child asphyxiated	..	1	..	1
Convulsions of mother during pregnancy, difficult labor	..	1	..	1
Mother in fever for ten weeks ante-partum	1	..	..	1
Unascertained	10	6	3	19
Total	22	20	7	49

In these tables we have analyzed, as far as possible, the causes in ninety-one acquired cases and forty-nine congenital cases of cerebral palsies.

Among the acquired hemiplegias the acute infectious diseases, including pertussis and pneumonia, play a very important rôle; a strikingly large number have come on during convulsions; in these cases the convulsions are not the initial convulsions of acute infectious diseases; the latter are considered separately in Table VI. The cases that have come on with fever and convulsions are noted in addition; but not wishing statistics to prove

TABLE VI.

Showing the relation of convulsions to the onset of the palsies.

Cases in which convulsions apparently preceded or were associated with the palsies occurring in—

Pertussis	3
Pneumonia	2
Scarlatina	1
Dentition	1
Vaccinia	1
Baptism	1
Fright	1
With beginning of menstruation	1
Fall on head	1
Gastro-enteritis	1
Total	13

Apparently immediate symptom of a focal lesion—in two cases.
Idiopathic and apparently only cause of palsy—in twenty cases.
Palsy occurring in ordinary epileptic seizure—in one case.

more than they should, it is but fair to add that in many of these cases fever may have been present, and that from among these twenty cases we may allow that several, if not all, show the onset Strümpell claims for his cases of polioencephalitis. We ask you also to note that in six cases of hemiplegia there was a distinct history of traumatism, that two hemiplegias and one diplegia were the result of cerebro-spinal meningitis; two cases of diplegia came on after measles—a fact of some importance. In

twenty-six cases of hemiplegia we were not satisfied with the statements elicited, and have therefore marked them as "causes unascertained." The table of causes in the congenital cases points a moral. In sixteen cases of the forty-nine, say in 33.3 per cent., there was some difficulty in labor, simple delay or instrumental delivery. The older writers—Little, Gaudard,[27] and others—refer to this cause, but have tolerably favorable statistics, Little mentioning but four cases; Wallenberg gives six of one hundred and sixty cases, and Osler nine of ninety-seven cases. The authors mentioned referred to hemiplegia only, and speak of forceps delivery as the element of danger. Our percentage is higher, because we include all forms of cerebral paralysis, and tedious labor as well as instrumental delivery. The moral is, that the forceps should be applied, if necessary, or delivery hastened by other means if protracted labor can be averted. A child's brain and skull have a wonderful power of resistance, but do not credit them with greater virtue in this respect than they really possess. The mother's life is by far the more important, but it is well to reflect that, other things being equal, she prefers a child that is neither paralyzed nor idiotic.

As regards the mode of onset of the congenital cases, it would appear paradoxical to say anything; but in several cases which were distinctly congenital, attention was first drawn to the disease by the appearance of convulsions at an early day. In these cases the convulsions are due to the same lesion or process which is responsible for the palsy. In the acquired cases convulsions preceded the onset of the other symptoms in thirty-six of eighty-three cases of hemiplegia, and in one case of acquired diplegia; loss of consciousness generally accompanied the convulsions. In six cases there was a distinct onset without loss of consciousness or convulsions; this occurred in four cases of left hemiplegia and in two of right hemiplegia; two of these left hemiplegias were distinctly syphilitic.

TABLE VII.

Analysis of six cases in which there was onset without loss of consciousness or convulsions.

Hemiplegia.	Age.	Cause.
Right with aphasia.....	Two years.....	Tonsillitis.
Right.....	Ten months..	Unknown.
Left.....	Two and one-half years....	Syphilis.
Left.....	Eight years.....	Unknown.
Left....	One and one-half years....	Syphilis.
Left..... .	Two and one-quarter years.	Fall on head.

Strümpell has made the onset with convulsions and fever a distinctive feature of his cases. The six cases referred to in our list answer to his description of infantile cerebral palsy as regards the hemiplegic form of paralysis. It is more natural to infer that they are not cases of polioencephalitis than that they are anomalous cases of that class.

On this subject of initial convulsions and loss of consciousness a word should be added, even at the risk of anticipating some inferences regarding the pathology of these palsies. Initial convulsions and loss of consciousness are distinctly cortical symptoms. They indicate very considerable cortical disturbance either by direct injury or by severe or sudden injury to any part of the brain, which would also imply disturbance of the cortex as of every other part of the brain. Relatively small injuries to the cortex, hæmorrhages, or cortical encephalitis will bring about loss of consciousness and convulsions; relatively large injuries to the interior need not exhibit these symptoms. The exception is in cases of embolism or sudden hæmorrhages, in which the suddenness of the shock disturbs the entire brain. This argument was urged by one of us[28] (S.) in a paper published some years ago, in which a typical case of capsular hæmorrhage was diagnosticated mainly on the line of argument just referred to; and in examining statistics of Wallenberg and others with reference to this point, we find that convulsions occurred only in cases of embolism anywhere, and in all cortical affec-

tions, however slight these may have proved to be. If all cases of infantile hemiplegia were cases of polioencephalitis corticalis, convulsions would invariably be present; but such is not the case.

The *form* of paralysis has been frequently referred to. Hemiplegia, double hemiplegia or diplegia, and paraplegia speak for themselves. Table I. gives the relative number of cases of each form in our list. Monoplegias are not included, although other writers had reported some such cases. We have seen but one case in which we were tempted to make a diagnosis of monoplegia, and this case gave distinct evidence on closer examination that the arm as well as the leg had been involved in the earlier course of the disease. The march of the disease resembles adult hemiplegia in this, that the leg recovers very much more quickly than the arm, for reasons that need not be given here. Under the heading of diplegia we have classed all cases in which both upper and lower halves of the body were involved ; in some of these one leg or one arm had so far recovered that the cases might have been interpreted as hemiplegia with an additional involvement of the other arm or leg ; but here again on closer scrutiny we became convinced that at one time all four extremities had been affected.

The involvement of the face is a matter of some doubt. The majority of the cases were seen at a time long after the recovery of the face. We can vouch, however, for the following statements. The face was affected in eleven cases of right hemiplegia, in nine of left hemiplegia, and in two cases of diplegia. In two cases (one of right and one of left hemiplegia) the leg was worse than the arm ; in all other hemiplegics the arm was the part more affected. In all diplegics the legs were more affected than the arms. Trunk and neck muscles were distinctly involved in two cases of diplegia. Strabismus occurred in one of right hemiplegia and four of left hemiplegia, and in three cases of diplegia.

It has been doubted whether aphasia followed the same

laws in the infantile hemiplegia as in the adult form, and Bernhardt[13] has seen fit to write at length on this special subject. Aphasia can only be said to be present if the hemiplegia comes on in an individual who had already acquired articulate speech; hence the forty-nine cases of hemiplegia and the twenty-one of diplegia which occurred before the age of two years are excluded from this consideration, and of the remaining fifty-six cases of hemiplegia the fewest dispensary patients or their parents could give satisfactory statements. We have records of seventeen cases of hemiplegia with undoubted aphasia. Of these seventeen, ten were cases of right hemiplegia and seven cases of left hemiplegia. Eight of these seventeen cases were observed by one of us (S.) in private practice. Of these eight, five had been distinctly aphasic, and three of the five were cases of left hemiplegia. This relatively large proportion of aphasia in cases of left hemiplegia is rather striking by contrast with the adult cases. As we grow older we appear to become more and more left-brained. In earlier years both hemispheres are equally entrusted, so it seems, with this highest faculty of speech. Bernhardt also comes to the conclusion that aphasia in children accompanies right as well as left hemiplegia. Prof. Osler notes aphasia in thirteen out of one hundred and twenty cases of hemiplegia, and only one of these with left hemiplegia. Wallenberg's statistics give forty-five cases in ninety-four right hemiplegics, and seventeen in sixty-six left hemiplegics, but he includes all sorts of speech disturbances, and the statistics cannot well be utilized with regard to true aphasia. Defective speech was noted three times in our cases of right hemiplegia, twice in diplegia, and in most of our eighty cases of idiocy.

The reflexes are either lively or exaggerated in the large majority of cases. The exceptions are noted in Table VIII.

The points to be noted are these: The exaggeration of reflexes occurs in the parts paralyzed or paretic. The great excitability of the reflex is often the best proof to be

TABLE VIII.

Showing the condition of the deep reflexes (knee-, elbow-, and wrist-jerks) in eleven cases in which they were not exaggerated.

Form.	Normal.	Diminished.	Absent.	Total.
Hemiplegia	4	1	3	8
Diplegia	1	1	1	3
Total	5	2	4	11

In all other cases the deep reflexes were exaggerated.

had of the former involvement of the leg or arm, although bilateral exaggeration of the reflexes in cases of unilateral paralysis must be taken into account; owing to the extreme contractures of the opposing sets of muscles, the reflexes can frequently not be obtained. This is true particularly of the ankle clonus and triceps reflexes. We must note, however, that in several cases the knee-jerks and other reflexes were subnormal and in some cases absent. In one case (No. 50) the quadriceps clonus could be obtained.

Disturbances of motion, such as choreiform, athetoid associated movements, are observed with unusual frequency after hemiplegia, and, as we have learned, also after diplegias in children.

In 105 cases of hemiplegia,	choreiform movements	occurred	in	6
" " "	athetoid	"	"	21
" " "	associated	"	"	15
" " "	rhythmical	"	"	1
" " "	ataxia	"	"	1
" " "	tetanoid contractions		"	1
In 24 cases of diplegia,	choreiform movements	occurred	in	1
" " "	ataxia		"	1
" " "	athetoid movements		"	1
" " "	nystagmus		"	2

These disturbances of motion after single and double hemiplegias have an unusual interest; they all point to some interference with the proper transmission of motor

impulses, either of the voluntary or of the inhibitory kind. The changes in the pyramidal tract are no doubt largely responsible for these peculiar movements, and that disturbances anywhere in the motor tract may bring about such aberrant movements is probable from the fact that these movements occur from the most diverse lesions, from lesions in the cortex as well as from lesions of the crus, as shown in an unpublished case of our own. Of associated movements we have seen a number of happy examples. In one instance the movements of the paretic arm and hand following the movement of the opposite side were so considerable that we were able to take tracings exemplifying this fact. Westphal[28] has offered the theory that in health there is a tendency for both upper extremities to act conjointly, but that inhibitory impulses permit unilateral and single action. In disease this inhibition is removed, and therefore the effort to move one arm or hand results in the movement of both sides. The irradiation of impulses, therefore, enters primarily into this question, but it seems to us to be open to doubt whether this irradiation occurs in the brain or in the spinal centres. In every case we must call in cortical inhibition to explain the separation of these irradiating nerve currents during normal life. Greidenberg has gone very fully into this special topic (*Arch. f. Psych.*, vol. 17).

Rigidity with contracture is one of the cardinal features of all these palsies, and the deformity resulting therefrom, as a rule, leads the patients or their relatives to seek medical advice. With the exception of the flaccid cases before noted, a very considerable number go on to marked contracture of one or several groups of muscles. Table IX. will give the main points, which need no further explanation except this, that attention should be drawn to the fact that cross-legged position of the legs occurs in diplegias and not exclusively in paraplegias, as is generally stated. Furthermore, that the contractures may be so extreme that the individual has a frog gait, as in the case of J. O., and that in three cases of hemiplegia there was distinct talipes

TABLE IX.

Showing the forms of contracture in the various cerebral palsies.

Form of contracture.	Hemiplegia.	Diplegia.	Paraplegia.	Total.
Flexors of elbow	29	..	..	29
" " both elbows	..	3	..	3
" " carpus and fingers	23	1	..	24
" " knee	5	..	..	5
" " both knees	..	2	..	2
Extensors of knee	1	..	..	1
Adductors of thighs (cross-legged position)	..	13	2	15
Talipes equino-varus	17	..	..	17
Double talipes equino-varus	..	5	2	7
Talipes equino-valgus	3	..	..	3
Talipes equinus	2	..	..	2

equino-valgus, while in all other conditions there was either simple varus or equino-varus position.

Why flexor and not extensor muscles are the seat of contracture, adductors and not abductors, is a puzzling question. It is probably due to the mechanical principles involved in the construction of joints.

A word in passing with regard to trophic disturbances. In many of these cases the circulation in the skin of the paralyzed limbs is as poor as in infantile spinal paralysis. There is often the same livid hue of the palsied limb. In all cases occurring early in life, and particularly in those of congenital origin, there is apt to be a very marked retardation of growth. This was most distinct in nine cases of hemiplegia, three of diplegia, and three of paraplegia. Atrophy of the thenar eminence was observed in one case of diplegia, coming on after a cerebro-spinal meningitis.

The association of epilepsy with infantile cerebral palsies is perhaps the gravest feature of these diseases. In our experience it is the one danger to be feared, and should be considered most carefully in any case of cerebral palsy in a child. In our list, sixty-two out of one hundred and forty cases were afflicted with epilepsy, or 44.3 per cent. of all cases. There were among the hemiplegic cases

forty-one cases of general epilepsy, nine of the Jacksonian type, and one case of petit mal (in all about fifty per cent.). In twenty-four cases of diplegia seven had general epilepsy (twenty-nine per cent.), one had Jacksonian epilepsy. In eleven cases of paraplegia four had general epilepsy (about thirty-six per cent.). The percentage of epilepsy in our own list differs but slightly from that given by Gaudard, Wallenberg, and Osler. In view of the high percentages conceded on all sides, there can be but little doubt that, taking all cases of ordinary epilepsy, a very fair proportion developed in connection with infantile palsies. Every sign of early palsy may have disappeared, while the epileptic taint remains. A case seen in private practice brings this out very forcibly:

E. B. (No. 81), girl, seventeen years of age; menstruated at age of eleven; third of four children. All others died of acute infectious diseases. Father was fifty-six years of age at time of birth of child. Mother was thirty years younger. The child has had epileptic attacks every three to four months for some years. No convulsions during childhood. Had been treated for ordinary epilepsy by many physicians; brominized for years. Closer inquiry revealed the fact that about four years ago the girl had an apoplectic attack, and on examination we found distinct evidence of left hemiplegia. Very marked weakness of left side; greatly increased reflexes; slight mental enfeeblement and marked anæmia. Bromides were discontinued, the patient put on tonics, and is doing well, at least as well as under bromides, which had evidently deepened the mental apathy.

It will be seen that the percentage of epilepsy was greatest in hemiplegia, but that it was a distinctive feature of all forms of paralysis here considered.

Taking into account the cortical origin of a large number of these cases of epilepsy, it was natural to expect a higher percentage of Jacksonian epilepsies, but in most of these cases the original focal lesion has disappeared, and a general atrophy and sclerosis have been established. General and not localized epilepsy is the natural result of this change.

Together with the occurrence of epilepsy we should note the large number of cases exhibiting some form of mental enfeeblement. The conclusions on this point are presented in Tables X. and XI., where we have attempted

TABLE X.

Showing the relation of mental defect to the age of onset of palsy.

Age at onset of paralysis.	Feeble-mindedness.	Imbecility.	Idiocy.	Total.
Congenital	6	15	14	35
Under three years	10	18	6	34
Three to five years	2	3	..	5
Five to ten years	2	3	..	5
Over ten years	1	..	..	1
Total	21	39	20	80

TABLE XI.

Showing the relation of mental defect to the form of palsy.

Form of mental defect.	Hemiplegia.	Diplegia.	Paraplegia.	Total.
Feeble-mindedness	16	2	3	21
Imbecility	31	7	1	39
Idiocy	7	8	5	20
Insanity (epileptic)	1	..	..	1
Total	55	17	9	81

to distinguish between feeble-mindedness and imbecility and idiocy. Eighty of one hundred and forty cases exhibited some form of mental impairment; sixty-nine were either in congenital cases or in those acquired in the first three years of life; fifty-two per cent. of the hemiplegic patients, seventy-one per cent. of the diplegic, and about eighty-two per cent. of the paraplegics were thus afflicted. The worst form (idiocy) was most marked in the paraplegias (forty-five per cent.), and least marked in hemiplegia, being found in only 6.75 per cent. of all cases. Here

again mental impairment can be said to be in proper relation to the extent of the cerebral lesion, which is presumably less extensive in cases of hemiplegia than in the other forms of cerebral palsies; though we must allow that the later development of many of the hemiplegias may have something to do with these results.

As a point of special interest we wish to add that Little, in nineteen cases of paraplegia in which there was some impairment of mental condition, found thirteen feeble-minded or idiotic (sixty-eight per cent.) and six (thirty-two per cent.) of good intelligence.

TABLE XII.

Analysis of stigmata degenerationis (exclusive of contractures) present in fifty-seven cases of cerebral palsy in children.

	Hemiplegia.	Diplegia.	Paraplegia.	Total.
Microcephalus	21	4	2	27
Leptocephalus	19	1	..	20
Macrocephalus	4	..	1	5
Marked cranial asymmetry	25	..	2	27
Marked facial asymmetry	19	..	..	19
Cranium proganæum	5	2	..	7
"Gothic" palate	9	1	..	10
Imperfectly developed teeth	10	1	1	12
Supernumerary teeth	..	1	..	1
Hirsuteness	..	1	..	1
Neuropathic ear	1	1	..	2
Strabismus	5	3	..	8

As for cranial defects,[29] the table appended will give all the information we have, though it is well to note that in most cases of hemiplegia there is a flattening of the skull on the side of the lesion, and in almost all cases of cerebral palsy most of the cranial diameters are below the normal averages.

In this review of clinical symptoms we have shown that, excepting the fact of paralysis, there are no symptoms peculiar to infantile hemiplegia that are not also found in diplegia and paraplegia. While the symptoms vary somewhat in degree in these different forms, all forms

have all symptoms in common, excepting those of the onset. It is a difference of degree, not of kind. It remains for us to prove whether or not a study of pathological conditions compels us to draw a distinction between these three forms, and whether or not hemiplegia, diplegia, and paraplegia respectively represent distinct morbid entities. We shall see that a variety of morbid lesions is to be found underlying these conditions, and that the same lesion or condition may in the one case be responsible for a hemiplegia, in the other for a diplegia, and so on. And furthermore, if definite forms of disease are to be diagnosticated, the diagnosis must rest upon other symptoms rather than the mere form of paralysis. This branch of our inquiry is beset with great difficulties. In the scarcity of autopsies we have shared the fate of other writers. In spite of our very large clinical experience, we have but two autopsies of our own.* But we claim this one advantage, that in both these cases the post-mortem findings were of very recent date—a great advantage, if we reflect that in most of the cases recorded in literature the conditions found were the final result of pathological processes which had continued for years, and which shed no light whatever upon the initial morbid lesion, and yet this is the salient point of the entire controversy. With the information gained by our macroscopical and microscopical studies, we have analyzed the records of one hundred and five autopsies, including our own. This list could have been enlarged if we had had access to the publications of Richardière [30] and some others. Our list, however, includes all the recent cases,† and the report of these will atone for the omission of those described by the older writers.

The first and the most conspicuous feature of this table

* A third autopsy has been added since the above was written (cf. note p. 186).

† Two cases of Kast, one by Hoven, Wallenberg, Salgo,[31] Langenbeck,[32] Fürbringer,[34] Zacher,[35] Hirt,[36] Peterson [cf. page 211 of this article], and Sachs.[26]

TABLE XIII.

Analysis of pathological findings of hemiplegia, diplegia, and paraplegia, based upon the most recent autopsies, including Wallenberg's and Osler's cases, but not those of Richardière or Audry.

Lesions.	Hemiplegia.	Diplegia.	Paraplegia.
Atrophy, sclerosis, and cysts	40	19	..
Porencephalus	2	4	..
Hæmorrhage	23	..	..
Embolism	7	..	..
Thrombosis	5	..	..
Agenesis	1	1	..
Tubercle	1	..	..
General cortical sclerosis	..	..	1
Total	79	24	1

Not including sixty-four cases of hemiplegia with porencephalus, and thirty-two cases of bilateral porencephalus collected by Audry.

is the prominence given to atrophy, sclerosis, and cysts. All these are terminal conditions and are almost useless for the determination of the initial lesion. Cysts are no doubt frequently due to hæmorrhages, and if this could be statistically shown the number of cases due to hæmorrhage would have been materially increased. Zacher has noted one case in which cystic formations occurred together with an osteoma, but the patient died thirty-three years after onset of lesion. Wallenberg has recorded an interesting case of cyst in the left peduncle.

Porencephalus is a secondary condition, and, although much has been written on this subject, we know little of its origin. In some cases it is probably the result of arrested development due to interference with fœtal circulation (Kundrat); in others it may have been the result of early encephalitis or even intra-uterine cerebral hæmorrhage. A number of different lesions may bring about a condition of porencephalus; moreover, the term has proved to be exceedingly elastic, and what one author has termed atrophy another has called porencephalus. Au-

dry,[37] for instance, has collected sixty-four cases of this condition, some of which in the other lists are labelled atrophy or atrophy and sclerosis. We are forced, therefore, to rely upon cases in which the initial lesion has not disappeared; and here the first fact that is brought out with great distinctness is that hæmorrhage, thrombosis, embolism, the conditions which give rise to adult apoplexy, are also found to be a frequent cause of the cerebral palsies of early life. There is mention in the table of a case of tubercle reported by Seeligmüller.[38] It is hardly fair to include such a case, for it was one of general tuberculosis in the course of which a hemiplegia appeared. While no other similar cases have been published, it is evidence of faulty logic that Seeligmüller should have inferred from this one case that latent tuberculosis is almost the sole cause of infantile hemiplegia. His case simply shows that hemiplegia may appear together with other symptoms, but there can generally be little doubt as to the nature of the process. It is for this reason that we have excluded from our list a case of our own in which the autopsy revealed a tubercle of the quadrigeminal region which had given rise to a hemiplegia in the course of the disease.

It may be surprising to find that polioencephalitis of Strümpell is not referred to. It is time that we should define our position with regard to this question. First of all, in order to distinguish this from two other lesions which have been termed polioencephalitis inferior (progressive bulbar paralysis) and polioencephalitis superior (nuclear ophthalmoplegia), let us speak of this as polioencephalitis corticalis. What proof have we that there is such a condition? Anatomical proof, none; we are willing to concede, however, that some of the many cases of atrophy and sclerosis may have been due to this polioencephalitis, but it is unfortunate for Strümpell's theory that all of the autopsies made soon after the onset of the disease have shown other conditions, and not a polioencephalitis. But let us be charitable or just, and say that

even these autopsies were not made in cases sufficiently recent. We must add, however, that cases which correspond very closely to the cases which Strümpell considered typical of polioencephalitis corticalis showed hæmorrhage, embolism, etc., of recent origin. Strümpell says, however, that not all cases of infantile hemiplegia need be due to this cause, and that all authors have misinterpreted his views. Is there no probability, then, that a few or any of the cases of infantile hemiplegia are cases of polioencephalitis corticalis? There is some circumstantial evidence showing that there is a brain lesion which would seem to be analogous to spinal palsy of children (poliomyelitis anterior). Möbius[39] gives the history of two children of one family, aged one and one-half and three years respectively, who were stricken down with fever, loss of appetite, and somnolence. One developed a typical poliomyelitis of the upper extremity; the other, spasmodic hemiplegia without aphasia. This is striking clinical evidence, though some might claim it to have been merely a coincidence. Another proof: Strümpell has but very recently reported two cases of adult apoplexy in which every one would have made the diagnosis—and indeed he made it—of embolic softening, but the post-mortem examination revealed a condition of encephalitis hæmorrhagica of the gray as well as of the white matter. Marie,[16] who is inclined to support Strümpell, expressed the opinion in 1885 that the encephalitis would attack the white as well as the gray matter, and thinks that this would not destroy the analogy with poliomyelitis, for in that condition the white fibres are sometimes involved. Jendrássik and Marie favor the perivascular (inflammatory) origin of the condition of lobar sclerosis, which they have carefully described. In view of all this, we venture the opinion that polioencephalitis corticalis may be the cause of some of the cases of infantile palsies; but, we add, not of the hemiplegia alone, for we have seen several cases, including one seen by the courtesy of Dr. Holt, in which all the symptoms were

those of Strümpell's disease, but there was a diplegic and not a hemiplegic form of palsy. In these cases the cerebral character of the symptoms was so distinct that a confusion with poliomyelitis was out of the question.

We insist that, until further pathological proof shall be forthcoming, polioencephalitis corticalis shall be diagnosticated last, not first. In a short paper, published some years ago in the *Journal of Nervous and Mental Disease*, one of us insisted that there was strong reason to think, by analogy with adult apoplexy, that the lesion might be the same in the infantile form. A short abstract of that history will show the analogy:

M. M. (Case No. 75), boy, two and one-half years old; one and one-half years previously pneumonia; tonsillitis with fever up to 102°. Four days after this, typical right hemiplegia and aphasia, positively without coma or convulsions. The onset was as typical as in the ordinary mild hæmorrhage into the internal capsule in the adult, and the progress of the disease and the mode of recovery (which became complete) were quite like what we see in adult cases.

In 1887 the evidence on this point was not so strong as it is now, and this leads again to the subject of thrombosis, hæmorrhage, and embolism. These vascular troubles were generally considered to be peculiar to advanced age, with the exception of embolism, which every one is ready to concede may occur at any age in which heart disease occurs. Thrombosis was thought frequent enough, from the occurrence of syphilitic endarteritis in children. Hæmorrhage in adults is attributed to the existence of miliary aneurisms. In the child miliary aneurisms have been found, and Prof. Osler[25 d] has described a large aneurism of the anterior cerebral artery occurring in a boy six years of age. If atheromatous degeneration of the arteries be less frequent than in adults, another condition is found to which Recklinghausen refers in his book on the "Pathology of Circulation and Nutrition," page 84. This is a fatty degeneration in the wall of the cerebral blood ves-

sels. Little notice has been taken of this, as indeed of all else that pertains to the vascular pathology of children.

Meningeal and cortical hæmorrhages are shown to be more frequent in children, while all other cerebral hæmorrhages are more frequent in adults. This will explain the more serious character of the symptoms in the young, and the more frequent occurrence of epilepsy and mental impairment in children. As the prognosis would be materially affected according to the cortical or cerebral character of the lesion, we have sought for the point of differential diagnosis between lesions so situated. To this end the occurrence of convulsions is specially to be considered. In the paper referred to it was stated: "Loss of consciousness is an extremely variable symptom; it seems to depend rather upon the quantity of blood effused than upon the area involved. Not so with convulsions. A convulsion, if it is anything, is a cortical affair, the result of cortical irritation." To which should be added that the irritation may be direct or indirect; the sudden shock imparted to the cortex by the occurrence of embolism anywhere in the brain is apt to cause convulsions, and a lesion in any part of the gray matter of the central nervous system may excite convulsions; but hæmorrhage over the cortex, even if slight, is apt to be accompanied by convulsions. Hæmorrhage into the interior of the brain, unless very large, and the condition of thrombosis—slow occlusion of a cerebral vessel—are more apt to be marked by absence of convulsions and possibly also by preservation of consciousness. In other words, if you can exclude embolism and sudden and very large hæmorrhage, and lesion of the lower gray centres, the absence of convulsions at the onset of an apoplectic attack is in favor of a lesion in the interior of the brain rather than in or upon the cortex. Of this I am more certain still, that whatever may be the symptoms accompanying the onset of an apoplectic attack, if the convulsions reappear after the initial symptoms, the lesion is cortical or else an additional insult has occurred. A possible exception might

be made in favor of thalamus lesion. These statements are corroborated by the results of recent autopsies, though only a small number of cases refer to all the facts needed. A case observed and examined post mortem by one of us (P.) is in point:

A. W., male, aged fifteen, bright at school, expert swimmer, at the age of eight or nine years was in the habit of diving a distance of twenty to thirty feet from a railroad bridge. Shortly after this he began to suffer from intense headaches, which gradually grew worse until he was twelve years old, when mental changes began to be apparent. Gradually loss of memory, confusion of mind, and steady progress toward dementia. At the same time moroseness, melancholia, morbid fear, and coprolalia. One month after admission into the Poughkeepsie Asylum he had a severe epileptic fit. Two days later right hemiplegia with constant right-sided hemi-epilepsy; became comatose. The clonic spasms of the right side continued at intervals of several days, but gradually became limited to the right leg alone. Four days later, death. Autopsy showed the outer dural surface to be normal; on the right side the subdural space presented a limited pachymeningitis hæmorrhagica interna, but merely a thin, organized, detachable stratum of long standing. Over the left side, however, the pachymeningitis was exceedingly widespread, extending over the whole surface of the hemisphere, both above and below, except in the anterior fossa of the skull. Over the convexity of the hemisphere the hematoma was very thick, consisting of strata of various ages, some of them undoubtedly dating from the beginning of his symptoms. Between two and three ounces of fresh coagula were spread on the convexity, especially in the Rolandic regions. The left hemisphere was greatly compressed; the brain substance itself seemed normal. The brain weighed thirty-five and a quarter ounces. The ventricles were widened and distended with clear serum. There was no disease of, or injury to, the cranial bones. There was no lesion of any kind in substance of hemispheres, ganglia, pons, or medulla.

Thrombosis occurs in a comparatively small number of cases. It is due either to the fatty change in the walls of the blood vessels, or to syphilitic endarteritis, of which we

have seen several cases, and Seibert[40] has described one, and is probably found in marantic cases. Gowers lays particular stress upon the thrombosis and occlusion of smaller cerebral veins, and thinks this the most important factor in the causation of infantile hemiplegia. All that we can say is that he must prove this to be true; the autopsies analyzed give no evidence whatever of this condition.

Thus far we have had reference chiefly to the pathological lesions of infantile hemiplegia. In diplegia nothing is said in our table of hæmorrhage, thrombosis, and embolism, though it is more than likely that some of the cysts referred to were originally due to clots, and the occurrence of meningeal hæmorrhages over both hemispheres, giving rise to diplegias, is well known. But these cases either die early before the form of paralysis is well established, or, if they live for many years, the initial hæmorrhage has disappeared and the secondary conditions only remain. You will please observe, however, that what is properly termed agenesis corticalis occurs in a case of hemiplegia reported by Kast, and in the case of diplegia reported by one of us (S.). The microscopical changes appear to have been very similar in both cases—in the one case unilateral, in the other bilateral, which accounts for the hemiplegia in the one case, for diplegia in the other.*

In the cases of Kast and Sachs there was no evidence of the agenesis having been due to early intra-uterine inflammation; in both cases there was a mere arrest of development and nothing more. The changes found in the case reported by one of us were limited to the large cells, in the cortex of which there was not a normal specimen to be found throughout the entire brain. There was no evidence of perivascular inflammation, and only very slight thickening of neuroglia tissue, with considerable retardation of development of the white fibres entering the gray matter of the cortex.

* A similar condition has been found by Jensen[41] in a case of idiocy.

The pathology of paraplegia has not been determined. We have but one case with autopsy, that of Förster,[42] in which case Birch-Hirschfeld found a general cortical sclerosis with probable descending degeneration.* Ross was induced to state, only a few years ago, that he thought the changes of the spinal lateral columns due to traction at birth might be responsible for spastic paraplegia. This is a bare possibility; but the occurrence of idiocy and mental enfeeblement in nine out of eleven cases of our own and in most of the reported cases, and the frequent occurrence of convulsions, make a cerebral origin much more probable.

The question arises whether it is possible to determine the morbid lesion in any given case, and we submit the following as the result of our clinical and pathological studies:

The form of paralysis is not the most important factor in diagnosis.

Acquired cases of hemiplegia and diplegia, but particularly the former, are apt to be due to the same causes that prevail in adult apoplexies, namely, hæmorrhage, thrombosis, or embolism. Meningeal and cortical lesions are more frequent. Absence of convulsions at onset probably points to an intra-cerebral lesion. Occurrence of convulsions at onset points generally to cortical lesions or to embolism anywhere in the brain. In a few cases affections of gray matter lower down in the central nervous system may also be attended by convulsions.

Frequently repeated convulsions after onset point with great certainty to cortical lesion.

Cases coming on after acute infectious diseases have been proved to be due to vascular derangement, particularly to hæmorrhage and embolism, but some cases may be due to a polioencephalitis corticalis; the onset with very high fever and convulsions makes the latter more probable.

Traumatism is an important factor in the causation of

* See note page 186.

meningeal hæmorrhage during early life, and particularly during the act of labor.

Congenital cases of diplegia and hemiplegia may be due to early meningeal hæmorrhage and possibly to an early encephalitis.

In acquired and congenital cases of hemiplegia and diplegia we may have either a condition of porencephalus or simple arrest of development. If acquired, there would seem to be the history of slow development of the symptoms without predisposing cause. If congenital, there may be a history of traumatism to mother.

In later life hemiplegia or diplegia associated with large amount of mental impairment and with contractures points to general atrophy, sclerosis, with secondary degenerations, or to a condition of porencephalus, the origin of which it is often difficult to determine.

In conclusion, a word regarding treatment. So far as prophylactic measures are concerned, we wish to repeat our warning to the obstetrician: Hasten protracted labor, for the skilful use of forceps and careful manipulation are less apt to do injury than the prolonged compression of skull and brain in the pelvic canal. In the acute cases the same principles which obtain in the treatment of adult cases should be applied.

Medical advice is generally sought for the relief of the secondary conditions—epilepsy, idiocy, and contractures. In the treatment of the epilepsy the usual remedies may be employed; above all, the bromides and chloral. But these should be administered with the utmost discretion, for in many cases the epileptic attacks are so infrequent that they do far less harm to the patient than the drugs, which increase mental stupor and are very apt to cause anæmia and malnutrition. The proper use of electricity and massage in the early stages will prevent to some extent the formation of extreme contractures. If such contractures exist, we place our reliance upon the orthopedic surgeon, who finds in these cases a very wide field for the exercise of his surgical and mechanical skill. In

cases in which the hand is so deformed as to be of less use than an artificial hand would be, amputation would be justified, provided there were no marked rigidity at the elbow. This has been thought of in the case of J. K., whose portrait has been shown. In one case, in this city, with extreme athetosis, the arm was amputated and the patient rendered more comfortable. This should be done only in the severest forms.

The question of surgical interference in cases of hemi-epilepsy deserves passing notice. In very recent cases, in which there is no prospect of spontaneous recovery and in which the character and site of the lesion can be accurately inferred, the idea of operation might be entertained, but it is wholly inadvisable in cases of long standing in which secondary conditions have been permanently established. The result in two cases, of Bullard[43] and Mitchell,[44] has not been encouraging.

Trepannation for the relief of severe headaches and local epilepsy may be attempted.

As for the condition of mental enfeeblement, much can be done by careful manual and mental training. This should be begun at an early day, and the results will be as satisfactory as in Bourneville's department of the Bicêtre, where the improvement is evident from the change in the facial expression of the patients on admission and after they have been under treatment and instruction for some time. In only two cases that we have seen of infantile palsies can we conscientiously state that there has been complete recovery.

REFERENCES.

[1] E. Henoch: Cf. also Lectures on Children's Diseases. London, 1889. New Sydenham Soc. Publication.

[2] J. von Heine: Spinale Kinderlähmung. Stuttgart, 1860.

[3] Little: Obst. Soc. Trans., 1862, vol. iii.

[4] M. Benedikt: Electrotherapie. Vienna, 1868.

[5] Cotard: Étude sur l'atrophie partielle du cerveau. Paris, 1868.

[6] Wuillaumier: De l'épilepsie dans l'hémiplégie spasmodique infantile. Thèse de Paris, 1882.

[7] Bourneville: Recherches cliniques et thérapeutiques sur l'épilepsie. Thèse de Paris, 1882.

[8] Hadden: Brain, vol. vi., 1883–1884, p. 302; also St. Barth. Hosp. Rep., 1882, and Brit. Med. Journ., 1882.

[9] Ross: Brain, vol. i. and vol. v.; also Dis. of Nerv. System, vol. ii., p. 465.

[10] Kundrat: Die Porencephalie. Vienna, 1882.

[11] Strümpell: Jahrb. für Kinderheilk. N. F., 1884, vol. xxii. Cf. also Textbook of Medicine, translated by Vickery and Knapp, p. 704; New York, 1887.—Ueber primäre acute Encephalitis. Deutsche med. Wochenschr., Oct. 17th, 1889.

[12] Ranke: Jahrb. für Kinderheilk., 1886; also Münch. med. Wochenschr., 1886.

[13] Bernhardt: Virchow's Arch., vol. cii., 1885.—Jahrb. für Kinderheilk., 1886, p. 384.

[14] Wallenberg, Jahrb. für Kinderheilk., 1886.—Arch. für Psych., vol. xix., p. 297, 1888.

[15] Kast: Arch. für Psych., vol. xviii., 1887, p. 437.

[16] Jendrássik and Marie: Arch. de Physiologie, vol. i., 1885; also Paul Marie, in Progrès médical, No. 36, 1885, p. 167.

[17] Gowers: Dis. of Nerv. Syst., Am. ed., p. 801, 1888. Cf. also Lancet, 1888, vol. i.—Medico-Chirurg. Trans., vol. lix., 1876, p. 271.

[18] Hoven: Arch. für Psych., vol. xix., p. 563, 1888.

[19] Sarah McNutt: Am. Journ. Med. Sciences, Jan., 1885, vol. i.—Am. Journ. Obstet., 1885.—Arch. for Pediatrics, Jan., 1885.

[20] W. Sinkler: Med. News, Philadelphia, 1885, vol. i.

[21] J. L. Smith: Journ. Am. Med. Assn., Feb. 25th, 1888.

[22] P. C. Knapp: Journ. Nerv. and Ment. Dis., 1887, vol. xiv., p. 480.—Bost. Med. and Surg. Journ., Nov. 22d, 1888.

[23] Lovett: Bost. Med. and Surg. Journ., June 28th, 1888.

[24] Gibney: N. Y. Med. Rec., vol. xxx., p. 393.

[25] Osler: (*a*) The Cerebral Palsies of Children. Monograph. Philadelphia, 1889.—(*b*) Also Phil. Med. News, July 14th to Aug. 11th, 1888.—(*c*) Am. Journ. Med. Sciences, 1885.—(*d*) Can. Med. and Surg. Journ., 1886.—(*e*) Alienist and Neurologist, 1889, p. 16, on Idiocy and Hemiplegia.

[26] Sachs: Journ. Nerv. and Ment. Dis., Sept. and Oct., 1887.—Intracerebral Hæmorrhage in the Young. Journ. Nerv. and Ment. Dis., Aug., 1887.

[27] Gaudard: Contrib. à l'étude d'hémiplégie cérébrale infantile. Geneva, 1884.

[28] Westphal: Arch. für Psych., vol. iv., 1873, p. 747.

[29] Fisher and Peterson: Cranial Measurements in Twenty Cases of Infantile Cerebral Hemiplegia. N. Y. Med. Journ., April 6th, 1889.

[30] Richardière: Étude sur les scléroses encéphaliques de l'enfance. Thèse de Paris, 1885.

[31] Salgo: Centralbl. für Nervenheilk., June 15th, 1889.

[32] Langenbeck: Quoted by Ranke in Jahrb. für Kinderheilk., 1886, p. 316.

[34] Fürbringer: Deutsche med. Wochenschr., 1889, xv., 67.—Berl. klin. Wochenschr., p. 16, 1889.

[35] Zacher: Arch. für Psych., 1889, vol. xxi., p. 38.

[36] Hirt: Path. and Ther. der Nervenkrankheiten, p. 223. Vienna, 1890.

[37] Audry: Revue de Médecine, June and July, 1888. Summary of Autopsies in 103 Cases of Porencephalus.

[38] Seeligmüller: Jahrb. für Kinderheilk., 1879. vol. xiii.

[39] Möbius: Schmidt's Jahrb., 1884, vol. cciv., p. 135.

[40] Seibert: Jahrb. für Kinderheilk., No. 22, 1885.

[41] Jensen: Arch. für Psych., vol. xiv., p. 752.

[42] Förster: Jahrb. für Kinderheilk., vol. xv.

[43] Bullard: Bost. Med. and Surg. Journ., Feb. 16th, 1888.

[44] Mitchell: Referred to by Osler. Monograph, p. 102.

In the above no attempt has been made to give a full bibliography. For this the reader is referred to Osler's monograph. The following articles, however, many of them recent and not included in Osler's list, deserve special mention:

Ashby and Wright: Diseases of Children. London, 1889.

J. W. Runeberg: Fall af sa kallad hemiplegia spastica infantilis. Finska läk.-sällskap. handl., 1884, xxvi., 261.

Bianchi: Defetto porencephalico. La Psichiatria, Naples, 1884.

D'Espine: Revue méd. de la Suisse Romande, March 20th, 1889.

Binswanger: Virchow's Arch., vol. cii., p. 13.

Schultze: Deutsche med. Wochenschr., 1889, p. 287.

Fr. Ziehl: Neurolog. Centralbl., July 15th, 1889.

Aufrecht: Ueber das Vorkommen halbseitige Lähmungen bei Oberlappen-Pneumonien von Kindern. Jahrb. für Kinderheilk., 1890, vol. xi., part iv.

Stephan: Des paralysies pneumoniques. Rev. de méd., 1889, No. 1, p. 60.

Lépine: De l'hémiplégie pneumonique. Thèse inaug., Paris, 1887.

Moncorvo: De la sclérose en plaques chez les enfants. Union Méd., January, 1884, p. 16.

Kirchoff: Eine Defectbildung des Grosshirns. Arch. für Psychiatrie, vol. xiii.

E. D. Fisher: Epilepsy and Hemiplegia of Children. Journ. of Nerv. and Ment. Dis., Sept., 1888.

S. Gee: On Hereditary Infantile Spastic Paraplegia. St. Barth. Hosp. Rep., vol. xxv., 1889, p. 81.

A. J. Richardson: Lancet, Nov. 10th, 1888.

H. Fritsche: Jahrb. für Kinderheilk., May 10th, 1889.

A. B. Marfan: Arch. für Kinderheilk., 1889, vol. x., p. 384.

L. Tenchini: Porencefalia grave bilaterale congenito. Ateneo med. parm. Parma, 1889, iii., 39.

A. Pilliet: Contrib. à l'étude des lésions histologiques de la substance grise dans les encéphalites chroniques de l'enfance. Arch. de Neurol., 1889, xviii., 177.

A. Weichselbaum: Porencephalie und mangelhafte Entwick. der recht. ober. Extremität. Ber. der K. K. Krankenanst. Rudolphstift. Vienna, 1888, 385.

Edwards: Liverpool Med.-Chir. Journ., July, 1888.

E. Powell: Brit. Med. Journ., June 30th, 1888.

E. M. Sympson: Cong. and Infant. Spastic Palsy. Practitioner, 1889, p. 114 ; also Feb., 1888.

G. Anton: Ein Fall von Mikrocephalie mit schweren Bewegungsstörungen. Wien. klin. Wochenschr., 1889, ii.

Ter Grigoriantz: Hémiplégie chez les enfants (103 pages). Paris, 1888.

D. Burgess: Manchester Med. Chron., 1888–'89, ix., p. 471.

C. J. Nixon: Trans. Roy. Acad. Med. Ireland, 1888, vol. vi., p. 21.

Aimé M. Gibotteau: Notes sur le développement des fonctions cérébrales et sur les paralysies d'origine cérébrale chez les enfants (135 pages). Paris, 1889.

Max Wolters: Angeb. spast. Gliederstarre, etc. Bonn, 1888.

Fauvelle: Bull. Soc. d'Anthrop. de Paris, 1889, xii., 227.

Worcester: Journ. Am. Med. Assn., 1889, p. 302.

Bramwell: Stud. Clin. Med. Edinburgh, 1889–'90, i., 156.

REMARKS ON THE FREQUENCY AND CHARACTER OF THE PNEUMONIAS OF 1890.*

By WILLIAM PEPPER, M.D., LL.D.,

PHILADELPHIA.

In attempting to give, in the few minutes at my disposal, a statement of the prevalence and peculiarities of pneumonia as it has presented itself in Philadelphia during the epidemic of influenza, I have tried to secure some reliable statistics by addressing nearly three thousand circulars to the physicians living in Philadelphia. To these I have had two hundred and seventy-two replies, each more or less full. Dr. Benjamin Lee, Secretary of the State Board of Health of Pennsylvania, has kindly given me the following returns, extending up to April 5th, as made to that Board, of the pandemic of influenza:

Total number of physicians giving returns		187
" " adults affected	19,868	
" " children "	8,258	
Total		28,126
Total number of nervous (as per returns)		5,275
" " catarrhal "		13,249
" " inflammatory "		4,976
" " deaths, directly caused	36	
" " " indirectly "	147	
Total		183
Immediate cause of death, bronchitis		4
" " pneumonia		86
" " phthisis		24
" " nervous symptoms		12

* An address delivered before the New York Academy of Medicine, April 17th, 1890, in a discussion on The Forms of Pneumonia which Complicate Epidemic Influenza.

Through the kindness of my correspondents, to whom I desire to return my sincere thanks for their co-operation in this inquiry, I am able to submit statistics of 35,413 cases reported, as above stated, by 272 practitioners. From my personal knowledge of the profession in Philadelphia, I am disposed to believe that these figures do not represent a fair average, but that I have secured returns from an undue proportion of those who treated a considerable number of cases; I believe, however, that, so universally prevalent was the disease, not less than two out of every three, or possibly three out of every four, of the eleven hundred thousand population of Philadelphia suffered from the disease in greater or less severity. The figures which I have secured are probably sufficiently large to warrant some deductions as to the mortality of the disease, directly or indirectly, and as to the number of cases in which pneumonia appeared as a complication. The number of deaths in the 35,413 cases is given as 257, or 0.72 per cent. Of these 257, 84 are reported as directly due to influenza without pneumonia. Out of the total number of cases of influenza, pneumonia occurred in 1,485, with a mortality of 173, or 11.65 per cent. There were also reported as occurring, independently of influenza, 2,471 cases of pneumonia, with a mortality of 324, or 13.11 per cent. Of these, 1,046 are given as croupous, with a mortality of 151, or 14.43 per cent., and 1,749 as catarrhal, with a mortality of 173, or 9.89 per cent.

It will be observed that of the 28,126 cases reported to the State Board of Health by 187 physicians in various parts of Pennsylvania, there were only 86 deaths from pneumonia. No mention is made of the total number of cases of pneumonia occurring, although it is probable that under the heading of "inflammatory cases" a considerable number of pneumonias were included. The individual experience of physicians has, however, varied greatly in this respect; thus, one of the most reliable of my respondents reports 1,000 cases, with only three cases of pneumonia, each case of pneumonia terminating

fatally. There were ten other fatal cases, and it may be suspected that in some of these a concealed pneumonia was the cause of death, or at least was co-existent. Another respondent, of the highest distinction as a clinical observer, reported 850 cases of influenza, with but five pneumonias and not a single death in the entire series. It is needless to say that the experience of those largely engaged in consulting practice is wholly without value from a statistical standpoint, and the same remark must be held applicable to the returns from hospitals.

A more complete notion of the great prevalence of pneumonia during the past season may be obtained from a chart, kindly prepared for me by Dr. Frederick A. Packard from the weekly returns of the Board of Health of Philadelphia. It is seen that during the period embraced between September 7th and December 28th there were, in 1888, 428 deaths from pneumonia, and during the same period of 1889 there were 453, or about an equal number of deaths from this disease; while during the period from December 28th to March 8th the deaths from pneumonia were, in 1889, 459; and in 1890, 810. There was also a notable, but not equal, difference in the returns of death from bronchitis, and it cannot be doubted that the cases of death from pneumonia in 1890 would be further increased by the fact that some such cases are included under the head of bronchitis. In addition to this, the total number of deaths returned as from influenza, without specific mention of pneumonia, is 138, and unquestionably a larger proportion of pneumonias are therein concealed. Finally, it will be noticed that during the five weeks from December 28th to February 1st, the number of deaths from pneumonia were, respectively, 71, 145, 182, 131, 82, a total of 611; while during the same weeks of the previous year the figures were, respectively, 44, 50, 38, 34, 55, a total of 221. The maximum number of deaths from pneumonia in one week in the winter of 1888-9 was 64—in the week ending March 8th.

In attempting to estimate the relative proportion of

the croupous and catarrhal varieties, the returns made to me (and which I must regard as more reliable than the aggregate returns to the city health office) would indicate that the catarrhal were almost twice as frequent as the croupous. It is no argument against this conclusion, as would be expected, when I say that Dr. Da Costa and myself saw at least as many cases of croupous as of catarrhal, in consultation, since this might be explained by the fact that the former were the more severe and dangerous (giving a mortality of 14.43 per cent. as against 9.89 per cent.).

In passing from this merely statistical return to the peculiarities of the pneumonias of the past season, I would first note the large preponderance of cases affecting the right lung. I should be inclined to say that this side was affected at least twice as often as the left. There was also an unusual proportion of cases of apex pneumonia where the entire upper lobe was completely consolidated without implication of any other part. As usual in such cases, there seemed to be a special predominance of cerebral symptoms.

It would appear that the association with pleurisy was even more constant and marked than usual. The side pain was often intense and sudden. The pleurisies were usually plastic; in one case, coming on with pneumonia immediately after exposure during convalescence, aspiration secured a pint and a half of pus on the third day and a few ounces of sero-pus from the pericardium on the following day.

I would offer merely as a suggestion resulting from personal observation that there has been an unusual prevalence of pericarditis, both plastic and purulent, in all probability, in some cases at least, associated with and preceding influenza. Corresponding prevalence of endocarditis has not been noted.

Jaundice has been observed in Philadelphia in a considerable proportion of cases, especially in those accompanied with pneumonia of the right lung. The evidence

as to the cause of jaundice is not conclusive. In the cases where careful examinations were made bile pigment was detected. In the following case (Case I.), which may be quoted in full as typical, there were hæmin crystals found in the first examination of the urine, which was made prior to the development of the jaundice, and subsequently bile pigment was detected. In the only cases where autopsies were made, the bile ducts seemed pervious, although, as is well known, a preceding catarrhal swelling may have subsided just before or after death. Jaundice was certainly a symptom of highly unfavorable omen in pneumonic cases; when it occurred merely in the catarrhal form in influenza not complicated with pneumonia, it was not necessarily grave.

Case I.—N. C., aged twenty-five, unusually vigorous and athletic, was in excellent condition when attacked by a very slight form of influenza. He was not confined to bed at all, and took no medicine, and did not consult a physician. He stayed in the house, but went to his meals, and in four or five days announced that he felt so well that he would go out after dinner to visit a friend a few blocks distant. The night was damp and raw. After sitting and conversing about an hour he was suddenly seized with a violent pain in the right side, which grew so intense that he fell upon the floor and vomited a quantity of blackish matter. It was not said, but it is not improbable that there was blood mixed with it. The vomiting was perhaps induced by brandy and ginger which was given to him. He was taken home in a carriage, and I saw him within an hour from the time of the attack, and found him with temperature over 105° and with the evidence of the congestive stage of pneumonia of the right lower lobe. He had a hypodermic of morphine to allay the pain, took aconite at short intervals during the night, with antipyrin and salicylate of sodium, each five grains. During the night pain was intense, the respirations were short and gasping, and the face wore an anxious expression. The hypodermic of morphine and atropine had to be repeated. The following morning the temperature had fallen to 103°, and at no time subsequently did it reach 105° till just before the fatal ending. Complete consolidation of the right lower lobe developed at the end

of forty-eight hours; the cough was painful, with rusty sputa; until the fourth day the temperature averaged from 102° to 103° and the pulse between 90 and 100. The stomach was irritable and retained only very small quantities of liquid. The tongue was heavily coated; the bowels were quiet. The urine was scanty, dark, and contained albumin and granular casts. He was treated with large doses of quinine by suppositories, and repeated half-grain doses of calomel until ten grains had been taken. At the close of this time, about the fifth day, there were several loose stools of light color. The patient seemed to be doing extremely well and frequently expressed himself as improving. By this time the rapid extension of disease began, and soon led to consolidation of nearly all of the upper lobe on the right side, while the middle lobe remained free. On the sixth day sudden and violent pleuritic pain developed on the left side, low down, followed by rapid consolidation of the lower lobe, extending upward and producing complete consolidation of the entire lung. Deep jaundice developed on the sixth day, and the urine showed the following changes:

January 12th, 1890. Acid, 1.031. No albumin, sugar, or casts. Evening—Acid, 1.027. No albumin, sugar, or casts. Many triple phosphate crystals.

15th. Acid, reddish color, 1.024. Distinct amount of albumin. No sugar. No casts. No red blood corpuscles. Teichmann's crystals.

16th. No hæmin crystals. Bile pigment detected by Gmelin's test.

17th. Acid. Trace of albumin. Bile pigment. No sugar. No casts. A few bile-stained leucocytes.

There were no cerebral symptoms, but a most singular persistence of a feeling of comfort, of strength, and of improvement within a few hours before death; with a perfectly clear mind and a natural tone of voice, he asserted that he felt well enough to sit up and walk across the room. The temperature rose progressively to 105°, the respirations steadily grew more rapid up to 50 to 60, and the pulse lost strength and grew very rapid. There was increased cyanosis. He died of heart failure on the beginning of the eighth day. No microscopical examination of the sputa was made. The autopsy showed typical croupous consolidation of the entire left lung, and, on the right side, of the entire lower lobe and of scattered patches in the apex. The middle lobe remained entirely

free, but was not fully expanded. The bronchial tubes were not obstructed; on both sides there were layers of plastic lymph; there was no pericarditis; no ante-mortem clots of valvular lesions. The liver was stained yellow, its tissues presented no marked change to the naked eye, and the bile ducts seemed pervious. No lesions of the gastro-intestinal canal. The blood seemed somewhat more liquid than normal.

Albumin in the urine was, I think, found in nearly all cases of pneumonia where it was carefully looked for, though not rarely there were no casts. Both hyaline and epithelial casts were found in some cases as early as the second day. It is clear that infectious nephritis was a frequent complication of influenza, and especially in cases with pneumonia.

Many cases of pneumonia were associated with an unusual degree of gastro-intestinal irritability. Vomiting frequently occurred spontaneously at the onset, and was readily caused by food or medicines during the course of the disease. Diarrhœa was also often easily caused. Both of these symptoms were highly unfavorable. Hæmorrhage from the bowels occurred in one case which came under my observation, where there had been marked abdominal distress and fulness. A case is quoted here in full, not only on this account, but as illustrating peculiar physical signs and nervous symptoms.

CASE II.—Mr. X., aged forty-six, of excellent health, but probably somewhat overtaxed and depressed in vitality, having recently suffered from a very severe carbuncle, had a sharp attack of influenza. His whole family suffered at the same time. He went out and attended to business while still relaxed and with a hard, dry cough. He exposed himself on the evening of January 8th, going out in a damp, raw air, and was attacked about midnight with chills, violent pains on the right side of chest, cough, and rapid development of fever. On physical examination there was found an extraordinary weakness of respiratory murmur over the right lung, especially over the two lower lobes. It was scarcely possible to hear the vesicular murmur, and crepitant râle was developed only after cough. Even on the left side there was no compensatory

exaggeration of the respiratory murmur. The case developed into typical lobar pneumonia, involving the entire posterior part of the right lung and the anterior part of its lower lobe. There were a few râles at the base of the left lung, but no pneumonia developed there. The temperature never became very high (103° maximum). The tongue was heavily coated and the stomach was very sensitive, but he was doing apparently well, when there occurred sudden collapse with marked heart failure and abdominal distress, which was explained soon after by several bloody discharges from the bowels. There had been a great deal of hebetude and mind-wandering before this, and this increased greatly after the pain extended, so that he was desperately ill until the twelfth day, after which improvement began and increased steadily until recovery. The urine showed:

January 10th, 1890. Acid, 1.018. Marked amount of albumin. No sugar. No casts.

11th. Acid, 1.021. Marked amount of albumin. No sugar. No casts.

13th. Acid, 1.018. Marked amount of albumin. No sugar. One hyaline and one epithelial cast in three slides.

15th. Acid, 1.017. Marked amount of albumin. No sugar. No casts.

The pyrexia deserved more critical study than could be given, owing to the excessive pressure of work. I would observe, in the first place, that there seemed to be a highly pyogenic state of the system widely prevalent throughout the community during the season. I am sure that such observations as the following must be familiar to all: One of my own children, aged five, seemed somewhat dull late in the afternoon, but did not appear at all unwell otherwise. I found his temperature 104°. In the case of a child, aged twelve, whose mother is a trained clinical observer, but who merely remarked that the child seemed feverish and dull, but did not need any attention, I found the temperature 105°. The pyrexia subsided in thirty-six hours with a development of no local lesion or the evidence of a specific influenza. Naturally, therefore, hyperpyrexia would be expected in complicated cases of influenza, and very many records have been furnished supporting this

view. A striking phenomenon was a very rapid rise of the temperature at the commencement of the disease; and not rarely this initial rise, continuing for the first eighteen or twenty-four hours, was followed by a distinct drop with no subsequent rise equal to the initial temperature. Temperatures of 105° and 106°, and in two cases of 107°, have been reported, the latter occurring toward the fatal close of the cases, one of whom was quite clear in his mind and the other fairly rational. On the other hand, in a few cases strikingly low temperatures were noted throughout the course of the extension of croupous pneumonia. In a case involving all of the left lung except the tip of the apex, and terminating in abscess, the initial temperature reached 102° for a brief period, but at no subsequent time was it above 101⅔° until hectic fever set in. It is proper here to call attention strongly to the fact that the vast majority of pneumonias occurred as a sequel rather than a complication, and that they were clearly traceable to exposure to damp and raw weather while the patient had a relaxed system and while slight pyrexia persisted, after the subsidence of the more marked symptoms of his influenza. Experience furnishes many instances of this fact. Both the cases above quoted are striking examples. It was not even necessary that the patient should leave the house to meet with this fate. In a marked case which I saw, a man aged thirty-five, heavily taxed with important professional work and depressed by the loss of a favorite child of three with apex pneumonia of a cerebral type, had an ordinary influenza, from which he improved so greatly by the fourth day that, in opposition to his physician's orders, he rose, dressed, and went to his library at some distance from his bed chamber. He soon had a chill, followed by temperatures of 105° and 106°, with right-sided pneumonia, rapidly developing delirium and deepening stupor, and with rapid and furious convulsions before death, which occurred on the fourth day. The practical lesson taught to all of us by this case must be embodied impressively in the records of this epidemic,

and in all future cases of influenza must serve as a most important guide in the management of this disease, especially in this state of convalescence.

Allusion has already been made to the nervous and cerebral symptoms attending the influenza-pneumonia of this past season. I am not aware that the cases of pneumonia which occurred unassociated with influenza presented any marked peculiarities in this respect, but, as should naturally be expected, the pneumonias which occurred during or after influenza proved otherwise. In the first place, they were often associated with exceptionally severe pain about the chest and with pains in different parts of the body. There is much evidence to show that these pains might be considered partly due to general neuritis or perineuritis of varying degrees of intensity. It is not now the time to discuss the question whether this affection of nerve trunks was of purely infectious origin or was due to atmospheric influences acting upon systems relaxed and sensitive to an almost unprecedented degree. It would seem, however, that the view of its infectious origin is strongly supported by many facts. The existence of such neural trouble has been made clear in a number of cases by muscular and sensory sequelæ. I refer to it partly in this connection because it has seemed probable to others besides myself that such a condition of the intercostal and respiratory nerves, and possibly of the pneumogastrics themselves, may be invoked to explain not only the chest pains, but the extraordinary weakness of the respiratory murmur noted in so many of our pneumonia cases. Another explanation of these striking phenomena that has been advanced is the hypothetical enlargement of the bronchial glands. I do not know anything in direct evidence in support of this view, but merely mention as of interest in connection with the matter that Dr. John Todd, of Pottstown, Pa., reports to me that in the cases of influenza occurring in that town there was diffuse enlargement of the lymphatics, even advancing to suppuration. We had no such reports from Philadelphia.

The cerebral symptom which attended the pneumonias here was dulness, at times amounting to deep hebetude or, in the latter stages of fatal cases, coma. In the following case (Case III.) this type of stupor was strongly marked for two days prior to the clear development of physical signs of consolidation. I am doubtful whether the case was one of influenza at all. It is more probable that it was a typical example of the ordinary infectious type of pneumonia, where, as is well known, the constitutional symptoms may precede by hours or even several days the development of the local lesions.

CASE III.—H., aged twenty-two years, an athlete in splendid condition, though overworked and exposed in a malarial district. He had felt unwell for several days, but continued at business. On Friday, December 6th, he felt very tired in the evening, but put on evening clothes and went to the theatre with only a light overcoat. He had fever during the night, and slept badly, but rose the following morning and went three miles to work, walking one and one-half miles. He had to go home, however, had a hard chill, and when seen that evening had a temperature of $104\frac{3}{5}°$, with evident congestion of the upper lobe of the right lung. He was stupid and heavy, but had scarcely any cough and made no complaint of pain, other than a slight soreness about the right nipple. Consolidation of the right upper lobe was not evident until December 9th. The temperature reached 105° on the second day, and for five days remained between 103° and 104°. It began to fall toward the close of the seventh day, and by the morning of the eighth day had reached normal. He continued in a state of deep hebetude until the sixth day, but without delirium. Throughout the whole course of the case there were only two slight coughing spells, and only on the fifth day did he raise any sputa, and then but two small portions of viscid, rusty-colored exudation. A careful microscopical examination of the blood showed no plasmodium. The entire upper lobe of the right lung and part of the back of the middle lobe were solid. When the temperature fell there was rapid resolution, with complete absorption without any expectoration. There was abundant herpes at both corners of the mouth. He took ten grains of quinine the first day, and on the second day

he took twenty grains in one dose in the morning. The stomach then became irritable, and he was treated with carbonate of ammonium, five grains every three hours, and antipyrin and sodium salicylate, four grains each about three times daily. On the seventh day he took five doses of five grains of quinine each. The rapid fall in temperature which followed on the next day was probably in large part a coincidence. Quinine was continued in decreasing doses. He made a complete and rapid recovery. The urine contained albumin, but no casts. Chlorides were present on every examination.

Delirium was frequently met with and varied much in type and degree. In many cases there were continual mind-wandering and delirium, and such cases were not rarely followed by emotional sensibility and weakness. Another type was of wild and maniacal delirium. This came on in some cases at an early stage, even as an initial symptom; it was sometimes persistent, and in not a few cases there was subsequent temporary dementia. Lastly, in other cases, especially in neurasthenic females or in anæmic and exhausted males, there was an excited delirium with garrulousness and even with hysteroidal phenomena. In other cases there was evident irritation, meningeal or cerebral, with wild excitement, passing into stupor with convulsions; or spinal, with stiffness and retraction of the head, muscle-soreness, and hyperæsthesia.

Finally, in regard to thoracic symptoms. Cough was at times almost or entirely absent (of course, generally in cases where cerebral symptoms were present). In some instances it was almost suppressed, with no adequate cerebral symptoms to explain it. Ordinarily it was a marked symptom and often severe, paroxysmal, and painful in both croupous and catarrhal forms. I regret to say that there seem to have been in Philadelphia very few careful microscopic or culture studies of the sputa; in the cases where search was made, the pneumococcus is reported to have been found. I am unable to report any new facts in regard to the pneumonic lesions. Certainly, in such cases as the first above reported, the lesions pre-

sent were to all appearance the typical ones of croupous pneumonia, and the same statement is made of other cases, with and without microscopic examination.

There is nothing especial about the sputum, except that, in cases with little or no cough, even extensive consolidation terminated in rapid absorption without expectoration (as in Case III.).

The physical signs were deeply interesting and in many cases were difficult to understand. To such an extent was this the case that I doubt not that in many examinations the diagnosis as to the extent and nature of pneumonia, or even as to the existence of pneumonia at all, was erroneous. The most evident cause of this difficulty was the excessive weakness of the respiratory murmur, as above mentioned, and the poor development of all auscultatory phenomena. Further, the co-existence of bronchial catarrh, even in croupous cases, often complicated the physical signs; and not only were the breath sounds weak in the early stages of congestion, but with complete consolidation the bronchial breathing was at times imperfectly developed.

A careful observer reports that he deliberately punctured a case of croupous pneumonia to establish a differential diagnosis, because the bronchial murmur was so feeble as to justify the suspicion that there might be a considerable amount of fluid. Although in many cases of the croupous type there was typical crepitus, it was often developed only on the deep breathing following coughing. In most cases, also, the râles were coarser and heard somewhat in expiration, though chiefly in inspiration, and in cases which were clearly croupous. After the exudation was absorbed, the affected area continued in some cases for a considerable time to give impaired resonance, weak breath sounds, and fine expansion crepitus.

Case IV.—Mrs. W. was seen in consultation with Dr. James Collins. She was a woman in good general health, when she was attacked with a slight chill, followed by

fever and coughing, which soon became associated with some pain, especially in the right side. There were also pains throughout the body, evidently due to influenza. The fever was of the regular type, with a maximum of $104\frac{3}{5}°$ and with daily variations of from 2° to $3\frac{1}{2}°$. At the close of the fourteenth day the temperature reached 100°, but did not remain down long, as symptoms of increased pulmonary trouble in the form of catarrhal pneumonia developed, and for the next five days the fever ranged between 100° and 103°, dropped again on the seventeenth day to $99\frac{2}{5}°$. The next two days presented an even greater range of remission, from 104° to $99\frac{2}{5}°$, and the following day rose from 100° to $104\frac{2}{5}°$; from the eighteenth day to the twenty-eighth day the fever ranged from 103° to 105°, reaching $105\frac{1}{5}°$ once. It then gradually fell, and reached 99° on the thirtieth day, and during convalescence did not again rise above 100°. From the thirteenth day to the thirtieth day the patient was so ill that her life was despaired of. Both lungs were extensively involved, but at no point was there any considerable area of dulness. The respiratory murmur was extremely feeble and in many places almost inaudible. At other points it was feebly blowing, with prolonged expiration. Percussion resonance on every light percussion gave very irregular results at different points of the pulmonary area. The cough was feeble and there was very little expectoration; it was not rusty-colored at any time. The pulse was extremely feeble and ranged from 96 to 136. The râles heard were irregularly distributed, and at certain points, both anteriorly and posteriorly, there were quantities of small subcrepitant râles. The respirations were not very rapid in comparison with the temperature, the average being from 24 to 26 a minute. The treatment consisted of nutritious enemata, small doses of morphine with quinine by suppositories, and by the mouth strychnine in large doses with aromatic spirits of ammonia. The patient made a complete recovery.

It is not designed to make any remarks in reference to treatment. The general type of the disease forbade the use of depletives or of depressing remedies. The general course of treatment which seems most useful may be inferred from the cases above reported.

THE RELATION OF PNEUMONIA TO INFLUENZA IN BOSTON.*

By FREDERICK C. SHATTUCK, M.D.,

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When I received the flattering invitation of your chairman to appear here this evening and speak on pneumonia as it appeared in Boston during the recent influenza epidemic, my first thought took the form of a wish that one more worthy of the honor had been selected; my second was how the subject could best be approached.

As it happened, though I have had cases both before and since, not a single one of my private patients had pneumonia during the influenza period. I saw a number of cases with other physicians, and had an unusual number in my wards at the Massachusetts Hospital; but consultation and hospital cases are not the most favorable classes for forming an estimate of the general severity of a prevalent disease. Either the disease is apt to be more severe, or the condition of the patients to be one of lowered vitality. The experience of any one individual may be very misleading. For instance, my hospital cases were of a very severe type, and the mortality was frightful—fifty per cent. I was thus led to believe that influenza-pneumonia is of special gravity—a belief which the general statistics of both hospitals, with a mortality of thirty per cent., apparently show to be erroneous.

After some thought it seemed to me that the best plan to follow would be to collect and analyze all the cases of

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* Read before The New York Academy of Medicine, April 17th, 1890, in a discussion on The Forms of Pneumonia that Complicate Epidemic Influenza.

pneumonia admitted to the two larger hospitals in Boston —the City, and the Massachusetts General—and to study the death returns from pneumonia and bronchitis at the City Hall. The City Hall statistics are not as trustworthy as those of the hospitals, inasmuch as any man, woman, or child can, in Massachusetts, style himself or herself a physician and sign a death certificate.

It has been assumed that the influenza period in Boston extended from December 10th, 1889, to February 1st, 1890, and the hospital cases of pneumonia are all included between these dates. The City Hall returns have been taken for December, 1889, and January, 1890, as the labor of compilation would have greatly increased by beginning at December 10th.

In the first place, allow me to consider briefly whether the past epidemic differed in any marked respect from those which have preceded it—a question not wholly foreign to the special subject of the evening. It seems to me probable that it did not. It was remarked in Boston, and also, if I err not, in other places, that the most striking feature of the disease was the prominence and frequency of nervous symptoms; the predominance of these, on the whole, over catarrhal symptoms, respiratory or abdominal. But I am inclined to think it possible that this is due simply to the fact that of late years our attention has been called more actively to the very important part played by the nervous system in disease of varied kinds. General prostration and violent pains in different situations are dwelt upon more or less, almost without exception, by those writing on the disease since the earliest times. It must be remembered, also, that a period of forty years has elapsed since the last widespread epidemic of influenza, and that this period has borne a rich crop of fresh knowledge in all branches of our science. Moreover, in this, as in previous epidemics, the most dreaded concomitant, complication, sequel, or feature has been pulmonary inflammation—a term which, in this connection, seems to me to demand some consideration, in

that it is inclusive of two processes quite different in origin and nature. It is not necessary before this audience to dwell on the distinction between lobar or croupous, and lobular, catarrhal, or broncho-pneumonia. But it may be observed that, while in most cases the two processes are easily enough differentiated at the bedside, under certain circumstances the case is far different. Croupous pneumonia does not always take a lobar distribution, sometimes involving only a portion of one or more lobes, central or peripheral, often terminating by lysis. Broncho-pneumonia may, on the other hand, especially with the aid of atelectasis, take a distribution resembling, to say the least, that of the truly lobar variety, particularly the double form. The number of cases in which a certain diagnosis of croupous pneumonia can be reached is much larger than obtains with the catarrhal variety, and the personal equation enters more largely into the diagnosis of the latter, one observer attributing the fine râles to capillary bronchitis, modified respiration and dulness, if present, to atelectasis; another observer inferring, perhaps, from the same signs the presence of lobular inflammation. These remarks are made because many writers speak of broncho-pneumonia as the commoner affection in influenza, the catarrhal respiratory form of which would certainly seem to pave the way for its origin. But, as far as the Boston experience goes, broncho-pneumonia was relatively rare. The diagnosis of broncho-pneumonia was made in seven hospital cases, five proving fatal but not followed by autopsy.

Two cases which I saw—one in consulting, the other in hospital practice—came clinically under the lobular rather than the lobar form. Both proved to be tubercular—one chronic but entirely latent until the influenza period; the other of the acute miliary variety. The latter was also interesting in that of ten sputum examinations, made with due care during life, nine were negative, and in the single positive one the bacilli were so scanty that some doubt was felt. Yet the lungs were crammed with miliary tu-

bercles, and there was much secretion. These cases led me to question whether two other fatal cases in young adults which I saw in consultation, and in which no consolidation could be detected during life and no post-mortem was made, were not also tubercular, the bacilli of a latent focus or foci finding the opportunity for rapid growth and dissemination in the bronchial catarrh and prostration involved in the influenza.

The next question to claim attention is the relation of pneumonia to influenza. Is pneumonia a concomitant or a sequel of influenza, or does it stand in some more intimate relation thereto? An analysis of the Boston cases does not enable me to answer this question satisfactorily to myself.

The only means which I have for estimating the frequency with which pneumonia and influenza are associated is furnished me through the kindness of Henry Saltonstall, Esq., the treasurer, and M. E. Parker, Esq., the agent, of the Pacific Mills at Lawrence, Mass.* Of 4,242 employees of the mills, 1,699 (or forty per cent.) were severely enough affected by the influenza to be kept from work an average of six days and a half each. Of these 1,699, eight had pneumonia, as far as can be learned; two were fatal. There is some liability to error here, but it is certainly small. This would indicate that less than one-half of one per cent. of fairly severe influenza was followed or accompanied by pneumonia.

Table I. shows that the number of deaths returned in Boston as due to pneumonia was greatly increased, especially in January, over the previous five years, while, as far as one can judge, the whole number of cases of the disease was increased in still larger proportion, inasmuch as the ratio of fatal cases in the two hospitals was nearly four per cent. less than the average for the corresponding period in the previous five years (Table II.).

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* Boston Medical and Surgical Journal, 1890, i., p. 251.

TABLE I.

Deaths from pneumonia reported at the Boston City Hall for the months of December and January, 1884–'85 *to* 1889–'90, *inclusive.*

	1884–'5.	1885–'6.	1886–'7.	1887–'8.	1888–'9.	1889–'90.
December	141	74	113	95	68	105
January	103	83	91	183	92	332
Total	244	157	204	278	160	437

TABLE II.

Cases of pneumonia admitted to the Boston City and the Massachusetts General Hospitals from December 10*th to February* 1*st, for the years* 1884–'85 *to* 1889–'90.

	1884–'5.	1885–'6.	1886–'7.	1887–'8.	1888–'9.	1889–'90.
Total cases	33	26	30	68	40	127
Deaths	14	4	12	26	13	38
Mortality	42+%	15+%	40%	39%	32½%	30%

It was a matter of general observation that the first cases of influenza were mild, and the correctness of this observation is fully borne out by Table I., which shows that the deaths from pneumonia in December, 1889 (one hundred and five), did not materially exceed the average

TABLE III.

Deaths from pneumonia reported at the Boston City Hall in December, 1889, *and January,* 1890, *by Weeks.*

Week ending—	Deaths.	Week ending—	Deaths.
1889:		1890:	
December 7th	9	January 4th	89
" 14th	13	" 11th	117
" 21st	17	" 18th	93
" 28th	36	" 25th	38
		February 1st	25

for that month during the preceding five years (ninety-eight). But Table III. shows that the number increased steadily, week by week, from that ending on December 7th, 1889, to that ending on January 11th, 1890, and then rapidly declined again. The increase was comparatively

slight until December 21st, when it became rapid, especially in the week December 28th to January 4th. In the last week of January the number of fatal cases fell again to about the average of the previous five years.

Table IV. shows the proportion of hospital cases in which pneumonia was preceded by grippe symptoms (forty-three per cent.). It will be observed that in twenty-nine per cent. such precedence distinctly did not take place, the number of these cases (thirty-six) being rather less than the average admitted to the hospitals the previous five years (forty-three). Under the head "doubtful" are classed those cases in which pneumonia came on so soon, or the initial symptoms were so ill-defined, that it cannot now be determined whether these symptoms were prodromal of the pneumonia or whether they belonged to the influenza. In the fourth class the histories are silent on this point.

TABLE IV.

Analysis of all cases of lobar pneumonia admitted to the Boston City and Massachusetts General Hospitals, December 10th, 1889, to February 1st, 1890, with reference to the precedence of grippe symptoms, localization, complications, and termination —lysis or crisis, or death.

	Preceded by grippe symptoms.	Not preceded by grippe symptoms.	Doubtful.	No history.	Total.
Total, number	55	36	26	10	127
" per cent	43	29	20	8	100
Single lobe, number	39	17	14	6	76
" " per cent	71	47	54	60	60
Unilateral, more than one lobe, number	6	11	5	4	26
" " " " " per cent	11	31	19	40	20
Double, number	10	8	7	..	25
" per cent	18	22	27	..	20
Complicated, number	13	11	14	5	43
" per cent	24	31	53	50	34
Recovery by crisis, number	15	7	3	..	25
" " " per cent	44	44	21	..	36
Recovery by lysis, number	19	9	11	5	44
" " " per cent	56	56	79	100	64
Death, number	11	14	8	5	38
" per cent	20	39	30	50	39

It therefore appears that in a number of cases, nearly large enough for an average year, pneumonia of the ordinary type of onset occurred, while in a larger number of cases the pneumonia first came on the scene after the invasion of influenza. It does not seem to me safe to draw any positive deductions from these figures, but they appear to suggest that pneumonia is not a primary manifestation of influenza. Influenza may be merely a predisposing cause of pneumonia, or the latter may be truly a secondary manifestation of the former.

Other points shown by Table IV. are the extent of the consolidation, the frequency of complications, the mode of termination of favorable cases, and the mortality.

In sixty per cent. of all the cases a single lobe was involved; in twenty per cent. more than one lobe on the same side; in twenty per cent. the pneumonia was double. In an indeterminable number of cases—a number larger, I am inclined to think, than in ordinary years, though I speak on this point with much hesitancy—the consolidation was irregular in distribution. That is to say, either a portion of only one lobe was involved, or else, in addition to complete consolidation of one lobe, patches of varying size were detected in another, the patients recovering or dying without further development of such patches.

In thirty-three per cent. of all the cases there were complications or sequelæ. Of these the most frequent were alcoholic delirium, phthisis, empyema, otitis, or facial erysipelas. Under this head there does not seem to be occasion for further remark. Of the sixty-nine favorable cases * in which this point has been determined, sixty-four per cent. ended by lysis, thirty-six per cent. by crisis. If the temperature fell to the normal point within thirty-six hours, the case was classed under crisis; in far the larger proportion of these the fall occurred within

* Some of the charts could not be found, so that the whole number of cases analyzed with reference to crisis and lysis is only sixty-nine, although eighty-nine recovered.

twenty-four hours. There is, as far as I know, no generally accepted standard of sharp distinction between crisis and lysis—a fact which is embarrassing in this connection. I may be wrong, but it is my impression that ordinarily the proportion of cases ending by crisis is larger than it was this year during the influenza period.

TABLE V.

Lobar pneumonia in the Boston City Hospital and Massachusetts General Hospital, December 10th to January 31st, 1890.

Age.	Cases.	Male.	Female.	Fatal.
Ten and under	2	2	0	0
Ten to twenty	14	9	5	2 (14%+)
Twenty to thirty	41	28	13	7 (17%+)
Thirty to forty	31	25	6	10 (32%+)
Forty to fifty	16	12	4	8 (50%)
Fifty to sixty	13	9	4	7 (54%−)
Sixty to seventy	1	1	0	1
Seventy to eighty	1	0	1	0
Unknown	8	8	0	3
Total	127	94 (74%)	33 (26%)	38 (30%)

Table V. shows the age and sex of the one hundred and twenty-seven hospital cases. Nearly seventy-five per cent. were males, and the mortality increased with each decade up to the fifth, remaining stationary thence to the seventh.

Table VI. compares the deaths from pneumonia and broncho-pneumonia returned at the City Hall in December, 1889, and January, 1890, with the average for the corresponding months of the previous five years, with reference to age. The table shows that—

1. In December, 1889, there was no increase under twenty or over seventy.

2. In December, 1889, at other ages, especially between twenty and thirty, there was a marked increase.

3. In January, 1890, the increase was very marked and general, reaching the maximum eighty to ninety, twenty to thirty, and fifty to sixty.

TABLE VI.

Fatal pneumonia and broncho-pneumonia reported at the Boston City Hall. Comparison between December, 1889, *and January,* 1890, *and the average of the same months during the previous five years, with reference to age.*

Age.	Average deaths, December, 1884 to 1888.	Deaths, December, 1889.	Per cent. increase.	Average deaths, January, 1885 to 1889.	Deaths, January, 1890.	Per cent. increase.
Ten and under	36.2	37	..	36	56	55
Ten to twenty	2.6	3	..	4	14	250
Twenty to thirty	8	26	225	8.8	43	388
Thirty to forty	10	22	120	17	54	218
Forty to fifty	11.8	28	137	15.6	57	265
Fifty to sixty	11 4	18	58	11.8	50	324
Sixty to seventy	11.2	15	34	13	38	192
Seventy to eighty	9	8	..	10.2	28	175
Eighty to ninety	3.8	3	..	2.2	13	500

TABLE VII.

Fatal bronchitis reported at the Boston City Hall. Comparison between December, 1889, *and January,* 1890, *and the average of the same months during the five previous years, with reference to age.*

Age.	Average deaths, December, 1884 to 1888.	Deaths, December, 1889.	Average deaths, January, 1885 to 1889.	Deaths, January, 1890.	Per cent. of increase.
Ten and under	37.2	23	36.8	29	..
Ten to twenty	0.6	1	0.4	3	650
Twenty to thirty	..	..	0.2	2	900
Thirty to forty	1	3	1.4	3	114
Forty to fifty	1.4	..	1.4	5	256
Fifty to sixty	4.8	4	2.4	8	233
Sixty to seventy	6	5	3.4	13	282
Seventy to eighty	2.4	4	5	13	160
Eighty to ninety	1.4	3	2.6	13	400

Table VII. treats the cases reported at the City Hall as bronchitis in the same manner. The table shows that—

1. Deaths from bronchitis in December and January under ten years were less than the average.

2. That no notable increase in December, 1889, occurred at any age.

3. That in January, 1890, there was a decided increase for every decade. The totals are, however, so small, and the liability of error in diagnosis so great, that the totals and percentages have no great value.

Are there, then, any conclusions which can be drawn from the data given above? Yes, conclusions can be drawn, but they do not seem to me of such a nature as to throw any really new light on influenza-pneumonia. They may be stated as follows:

1. Pneumonia was unusually prevalent in Boston during the height of the influenza epidemic, about the middle third of the visitation.

2. The statistics of the Pacific Mills indicate that less than a half per cent. of those severely attacked by influenza acquired pneumonia.

3. Broncho-pneumonia was rare in the hospitals.

4. The pneumonia mortality rate was probably not increased, perhaps diminished, as compared with that of the five previous years.

5. The number of cases of pneumonia not preceded by grippe symptoms was about the same as the number of pneumonias in an average year.

6. Pneumonia followed grippe in so large a number of cases as to show some sort of connection between the affections.

7. In sixty per cent. of the cases a single lobe only was involved.

8. Two-thirds of the cases terminated by lysis.

9. Pneumonia was three times as frequent in males as in females, and the mortality rate increased with each decade.

10. The most striking increase in the urban deaths from pneumonia was, on the whole, between the ages of twenty and sixty and eighty and ninety. The increase under ten was slight.

11. The gross appearances in nine cases examined after death were not specially noteworthy.

In conclusion, I wish to express my thanks to Dr. Lyman, Dr. Mason, and Dr. G. B. Shattuck, of the City Hospital, and Dr. Fitz, of the Massachusetts, for kindly affording me the full use of their records, and to Dr. J. P. Clark for indefatigable zeal in collecting the facts embodied in my paper.

REFLEX CHOREA.*

Abstract of Remarks by Dr. A. JACOBI.

He said there was a large number of conditions in which chorea was present, but not always in relation, perhaps, of reflex. It was seen in various cerebral states, as apoplexy, tumors in different parts, inflammation of the meninges, of the cerebral arteries, of the spinal cord, in sclerosis and softening of the brain or cord, in bilateral, sometimes unilateral paralysis, in epilepsy, etc. Chorea was also due to blood diseases—to rheumatism, anæmia, general exhaustion—and was sometimes found during the process of growth. It might be due to direct nerve influence, as fright or neuritis, but other cases were of reflex nature in the restricted sense, and of this kind he would speak more particularly of nasal reflexes. Among other disorders of which it was a reflex, he named those pertaining to the digestive system, the sexual organs; one case on record was due to fissure of the anus, a number to cicatricial contraction, etc.

Nasal Reflex Chorea.—During the past ten years much attention had been given to diseases which were considered of reflex nasal origin. Among writers who had made mention of reflex nasal chorea were Seiler, McKenzie, Hack. The speaker had published a paper on the subject in 1886. He might say that for dozens of years he had seen many cases of the following description: A child, three, seven, or ten years of age, would show a number of symptoms which to most people did not appear those of

* Remarks made before The New York Academy of Medicine, May 1st, 1890, in a discussion upon The Relation of Peripheral Irritation to Disease, continued from March 20th, 1890.

any morbid condition, but simply the result of bad habits, for which it was punished. The parents were told that with puberty or sooner the child would get well, but instead it often grew worse. The symptoms consisted in local convulsive movements, as twitchings about the face, winking of the eye or eyes, horizontal and vertical frowning, twitchings of the angles of the mouth, shrugging of the shoulders, jerking of the arms, sometimes throwing about of the arm, hawking, slight cough, some nasal catarrh. This lasted a number of years, being better perhaps during the warmer months. On examination the nose would be found the seat of catarrh, the mucous membrane thickened, considerable discharge present, glands of the neck swollen, particularly near the angle of the jaw ; sometimes there was ozæna, one or both nostrils more or less obstructed, sometimes deviation of the septum, in every case a good deal of pharyngeal catarrh, the tonsils hypertrophied and presenting a number of follicular cavities, in a number of cases a little œdema. Since publishing his paper in 1886 he had seen at least four cases of general chorea due to local conditions of this nature, and altogether he had seen at least a dozen cases in which all the members participated in the choreic movements, which had no other cause than chronic naso-pharyngeal catarrh, deviation of the septum, ozæna, hypertrophy of the tonsils, etc. This form of chorea differed from usual chorea in that the convulsive movements were not made worse by voluntary attempts to use the muscles.

Treatment.—These cases were likely to last a long time because not receiving proper treatment. The movements began in the face, therein differing from ordinary chorea, and were likely to be accompanied by mental dulness rather than by mental irritability as in the usual form. Local treatment was called for, but he had found milder measures than those often adopted of recent years suffice. Salt wash, with a small proportion of alum, would sometimes be sufficient; in some cases the galvano-cautery was called for. A cure could not be effected until the

enlargement of the tonsil had been overcome. While he did not wish to exaggerate the importance of local reflexes in the causation of this or other diseases, yet attention should be called to them if we would effect a cure in certain cases.

THE TRUE RELATION OF DISEASED CONDITIONS IN THE UPPER AIR PASSAGES TO THE SO-CALLED NASAL REFLEXES.*

By F. H. BOSWORTH, M.D.,

NEW YORK.

WHILE glancing through some old medical books a few evenings since, I came upon the following passage from the writings of that "learned and renowned doctor," Lazarus Riverius: †

> "That Rheum is called Catarrhus which doth fall
> Upon the Breast, upon the Jaws we call
> It Branchus, Coryza through the Nose doth fall."

"When the humor flows upon the Breast, the disease keeps the general name of a Catarrh or Defluxion; when it falls upon the Jaws, and Aspera Arteria or rough Arteries, it is called Branchus, Raucedo, or Hoarseness; when it flows into the Nostrils it causeth not only a Coryza or Murrh, but Ozæna or Polypus. But in other parts it produceth various effects; if it fall upon the Nerves, it produceth a Torpor or Numbness, a Palsie, Convulsion, Trembling; if in the Ears, Deafness, Swelling; if in the Eyes, Ophthalmy or Inflammation, Tears, Blindness; if upon the Uvula or Palate, Swelling, Looseness, or Laxity, or Ulcer; if in the Throat, the Quinzy; if on the Lungs, the Pleurisie, Inflammation, or Imposthumation, Cough, Shortness of Breath, Spitting of bloud, Consumption; if into the Stomach, Vomiting, Want of appetite; if into the Bowels, it causeth Diarrhœa and Dysentery; therefore it is rightly conceived that the greatest part of diseases that trouble Man's body have their original from the Head."

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* Read before The New York Academy of Medicine, May 1st, 1890, in a discussion upon The Relation of Peripheral Irritation to Disease, continued from March 20th, 1890.

† Culpeper's translation, London, 1678, p. 52.

This is the result of the studies of a very keen observer, who practised general medicine, be it remembered, and was not a specialist, some two hundred and fifty years ago; and I bring forward this quotation to-night, as possessing a somewhat curious interest, and as showing that the idea that all diseases come from the nasal passages, or the head, as Riverius terms it, is by no means a new one. I am not, however, disposed to go quite so far as our ancient friend, for I am not one of those who believe that the nose is absolutely the direct cause of the large number of diseases which are now recorded in the category of nasal reflexes. Furthermore, I think it still an open question whether these diseases, when met with in connection with intranasal disorders, should properly be classed as reflexes. This term is an exceedingly vague and indefinite one, which we use oftentimes to explain certain symptomatic manifestations, when the real connection between the primary disease and the irregular or unusual manifestation is obscure and hidden. Certainly many of the affections which we are accustomed to term as reflex, I think, properly must be regarded as directly symptomatic.

Thus, for instance, we not infrequently hear cough spoken of as a reflex disturbance due to disease of the nose or naso-pharynx. A cough, in these cases, is purely a symptom of a secondary complication of a catarrhal nature in the larynx or trachea, due directly to the disease in the parts above. Frontal headache is oftentimes spoken of as a reflex disturbance due to intranasal disease. In certainly many instances a frontal headache can be better explained by regarding it as due to the fact that the turgescent condition of the blood vessels of the nasal cavity exercises a certain amount of pressure on the terminal filaments of the first branch of the fifth pair of nerves. Epiphora is often spoken of as due to reflex disturbance from intranasal disease. This is unquestionably to be explained by the fact that the catarrhal inflammation of the nasal mucous membrane extends into the lachrymal sac, producing occlusion. A still further extension of the inflam-

mation may result in involving the conjunctiva in an inflammatory process which is in no way to be regarded as a reflex result.

I bring forward these points simply to illustrate what I regard as, to a certain extent, a misuse of the term "reflex."

The main interest in the discussion to-night, as far as I am asked to take part in it, concerns, I believe, the question of hay fever and asthma. I have already written at considerable length on this subject, and therefore shall be somewhat brief in what I have to say to-night, for fear of repeating what has been already said.

I do not believe that intranasal disease is the cause of every case of asthma or of every case of hay fever. As far as I remember, I have never made any direct statement to this effect, although the tone of the discussion at the last meeting of the Society (in which I was quoted and referred to) seemed to point in that direction. My position in this matter is based on a pretty large observation of both hay asthma and perennial asthma, as it has manifested itself in the somewhat large number of patients which have been under my personal care, and is the direct teaching of the clinical histories of these cases, and especially of the results of treatment. My position, briefly stated, is as follows:

1st. The special morbid lesion which gives rise to a paroxysm of perennial asthma is a dilatation of the blood vessels which circulate in the mucous membrane lining the bronchial tubes, the result of a vaso-motor paresis. This vaso-motor paresis differs from inflammation in that, while constituting apparently its first stage, it shows no tendency to go further. Muscular spasm, therefore, according to the old teaching, plays no part whatever in producing an asthmatic attack.

2d. There are two predisposing causes of asthma. First, that condition of the general system which we call neurosis, under the influence of which an individual becomes liable to vaso-motor disturbances in one portion of

the body or another. This I regard as good a definition, from a pathological point of view, as we can give of what we call neurosis. I can only say that, as far as clinical observation teaches us, the one pathological lesion which characterizes the direct manifestation of a neurosis is a vaso-motor paresis in one portion of the body or another. In asthmatics this vaso-motor paresis involves the blood vessels which circulate in the mucous membrane of the bronchial tubes. The second predisposing cause of asthma is a chronic inflammatory process involving some portion of the upper air tract. In all chronic inflammation the prominent feature is vascular dilatation. The whole mucous membrane of the upper air tract is very closely and intimately related, one part being in quick and close sympathy with another. A hyperæmia of the blood vessels of the nose shows a marked tendency to be followed by a similar condition of the mucous membrane of the bronchial tubes. This, I think, is necessarily a corollary of what we now recognize as the great respiratory function of the nasal passages, by which the temperature and moisture of the inspired air are nicely regulated and adjusted before its entrance into the bronchial tubes.

If I have made clear these two propositions of what a neurosis is, and of how closely related the two portions of the respiratory tract are to one another, I think the dependence of an asthma on a nasal condition can be clearly understood.

Asthma, as I said before, is not in all cases caused by an intranasal condition, but an intranasal morbid condition plays an exceedingly important part in its development. Given an individual with hypertrophic rhinitis, or any other obstructive lesion in the nasal cavity, who at the same time is of a well-marked neurotic habit, and I think you have a subject who presents exceedingly favorable conditions for the development of an asthma, especially if the nasal condition in this individual has existed for a sufficient length of time as to have developed a mild tracheal or bronchial catarrh. On the other hand, take an individual

of a mild neurotic habit, with healthy air passages, or, again, an individual of a notably phlegmatic temperament, with a very advanced condition of chronic inflammation in his nasal passages, and the development of an asthma would be a most curious phenomenon, according to my views and observation.

I hold firmly, therefore, to the opinion that in a diseased condition of the nasal cavities, of a chronic inflammatory character or of an obstructive character, we have a powerful predisposing cause of asthma. I go no further than this, and, as far as I remember, I never have made a statement which could be interpreted as meaning more than this. Dr. Robinson, I think, in the discussion at the last meeting, does me something of an injustice when he quotes me as saying that I have "never known a case of asthma to occur in other than an obstructive lesion of the nose or upper air passages." In my original paper, "Asthma, with an Analysis of Eighty Cases," * I made the assertion that "a large majority, if not all, cases of asthma were dependent upon some obstructive lesion in the nasal cavity." In a later publication I made the assertion that the lesion in the nasal cavity was always of an obstructive character. The way in which Dr. Robinson quotes me, I think, is a trifle misleading, as one would infer that I made the somewhat extravagant claim that no case of asthma could occur without an obstructive lesion in the nose. I simply say that I believe a large majority of cases are dependent upon an intranasal lesion, and when such lesion exists it is obstructive. I have never seen a case of asthma in atrophic rhinitis, and should consider it a most curious phenomenon if such were to present itself.

When I say that asthma is dependent on nasal disorders, I do not say that all cases are caused by it. I assert merely that there is a close relation, and this is evidenced by the report that I previously made, in a manner that cannot be questioned. Of the eighty cases reported the

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* American Journal of the Medical Sciences, September, 1888.

treatment was largely intranasal. Of these cases, forty-six were cured, twenty-six were improved. Now, I possibly may have been carried away by a certain degree of enthusiasm in reporting these cases. I take it few of us, in endeavoring to establish a somewhat novel premise, are capable of making our reports without a certain degree of bias. In my eighty cases I endeavored certainly to make my reports with fairness and candor, and the results were obtained by a somewhat extensive correspondence with the patients who had passed out of my observation, and none were reported, as far as I know, who had not responded to my letters or had not been personally seen. Now, if in eighty cases forty-six can be cured and twenty-six improved by the local treatment of the nose, there can be no question that we have established the fact, to my mind beyond controverting, that a very large majority of cases, if not all, are dependent upon an intranasal lesion; and by this dependence I do not mean caused by, but that the two affections are so closely related that the asthma can be very materially affected and controlled by medication to the nose. This fact certainly has been established by numberless observations. If a paroxysm of asthma can be arrested by an application of cocaine in the nose, as I have seen it over and over again, the relationship between the nose and the bronchial tubes is clearly established and without question. If this can be done in a large number of cases, this relationship is demonstrated to be still more intimate. If the permanent control of a chronic inflammation of the nasal mucous membrane cures asthma, we have not only shown this relationship in a very clear manner, but we have presented to the profession a method of treating asthma which is not only definite and certain in its action, but most satisfactory in that the treatment is directly under our eye, and what we accomplish can be seen and observed. In other words, in place of a somewhat obscure pathological condition, there are presented certain physical manifestations which are under ocular inspection, and the treatment of which also

is under ocular inspection, and our success is carefully estimated by the physical improvement in local conditions which we observe from day to day.

As regards hay fever, this I consider as practically one and the same disease with asthma, and to be treated in the same way. On this point I have already written so much at length that I do not feel at liberty to repeat it here.

One word as regards certain nervous diseases which have been classed among the nasal reflexes. I refer especially to chorea and epilepsy. I have seen nothing in my own practice which warranted me in the belief that epilepsy should be regarded as a nasal reflex, although I do believe that an intranasal condition is capable of proving a marked source of irritation in many of the nervous affections, and that an epilepsy may be aggravated by an intranasal morbid lesion, the removal of which may to a certain extent modify the appearance of the convulsions. That an epilepsy has ever been cured by intranasal treatment I think is open to very serious question. The only instance of this kind I recall is that of Roe, read at the last meeting of the State Medical Society. I am disposed, however, to think that it is too early to definitely estimate the results in this case.

Of chorea I have seen a number of instances. Only three, however, have been sufficiently long under my care to warrant my giving a definite opinion concerning them. One of these was a young lady from Havana, aged twenty, with chorea of the larynx, which was characterized by the most distressing bark, perhaps, that I have ever listened to in my office. Dr. Starr saw this case in consultation, and will readily recall it. She was entirely cured by intranasal treatment, and now, at the end of nearly two years, has had no recurrence. Another case was that of a boy, aged seventeen, referred to me by Dr. Janvrin, with hypertrophic rhinitis and deformed septum, in connection with which there was well-marked choreic movements, which disappeared permanently on the relief of the intra-

nasal condition. A third case is a young man, aged sixteen, still under treatment. This was a marked case of chorea, and rather a distressing one. The improvement, however, has been fairly satisfactory, and I am disposed to anticipate an entire relief, if the young man persists in his treatment. Dr. Loomis saw this case a few weeks ago, and, as I understand it, gave an absolutely unfavorable prognosis. I hope yet to convince Dr. Loomis that this unfavorable opinion was premature.

I do not mean to suggest any especial connection here, reflex or otherwise, between chorea and disease of the upper air passages, for I do not claim any such connection to exist. The improvement in these cases is to be explained by the fact that the removal of morbid conditions in the air passages of young patients is often followed by marked improvement in the general health. In other words, I consider the successful treatment of catarrhal disorders in the young as one of the very best of tonics. This, I take it, is the prominent indication for treatment in chorea.

Before closing may I add one word in regard to a criticism which Dr. Robinson makes on my recent volume on "Diseases of the Nose and Naso-Pharynx," wherein he says that, on page 525, the statement is made that "the secretion from the nasal passages of the pharynx never makes its way into the laryngeal cavity under any circumstances." Comparing this with the passage on page 550 of the same volume, he claims an inconsistency, in that I make the assertion that "cough is present in certain cases, especially if there is much secretion which has made its way down the pharynx into the larynx, exciting laryngitis."

The quotation from page 525 is from the article on naso-pharyngeal catarrh, while that on page 550 is in the article on adenoid disease of the naso-pharynx. In the former case the disease is one of adult life, in which I do not believe the secretions ever make their way into the larynx; in the latter instance I was discussing a disease

of child life, wherein the cough may unquestionably be due to the secretions making their way into the larynx.

I consider myself guiltless of any inconsistency whatever in the statements, and see no reason to change the views there expressed. These points are scarcely germane to the subject under discussion, but since Dr. Robinson has, in rather an elaborate manner, brought them in, I think I am entitled to equal prominence in correcting his somewhat unnecessary charge of inconsistency.

I think the weight of his charge, however, is more than overbalanced by the compliment he pays me, in that the detection of this so-called inconsistency shows conclusively that Dr. Robinson has given my work a most careful reading. I can only trust that this may eventually inure to his great profit.

THE RELATION OF PERIPHERAL IRRITATION TO DISEASES OF THE WOMB AND ITS APPENDAGES.*

By CHARLES CARROLL LEE, M.D.

Mr. President and Gentlemen, Fellows of the Academy of Medicine:

PERHAPS in no part of the body is the relation of existing disease and peripheral irritation more frequent and complex than in the womb and its appendages. Hence I have found the subject delegated to me by the chair in this discussion no easy task. This naturally results from the greater sensitiveness of the nervous system of women than that of men, and the extreme potency of the uterus as a factor in the production of reflex irritation in other organs.

Now, in the brief consideration of the subject which I am about to submit, moderation and accuracy of statement are of prime importance. For, if we are implicitly to credit enthusiastic writers on gynæcology, or to attach much import to the assertion of hysterical patients, there is no "ill that human flesh is heir to" (when that flesh is of the feminine gender) which may not be of this nature. Therefore I shall at once dismiss the thought of discussing the more recondite conditions of structural disease developed in distant organs by persisting disease of the womb—of phthisis, for instance, resulting from uncured laceration of the cervix—and at once ask your attention

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to a few of the well-marked neuroses, or, if the term be preferable, the hystero-neuroses, which best illustrate my subject.

At a recent meeting of The Academy, Dr. M. Allen Starr stated the etiology and the physiological significance of the reflex neuroses with a completeness that leaves nothing to be added from that standpoint; and my distinguished predecessor, Dr. Jacobi, has this evening exhausted the subject of chorea, which is one of their most striking illustrations.

Omitting, then, chorea, of which many cases are recorded as intimately dependent upon uterine and ovarian disease, I may say that the most frequent illustrations of peripheral irritation resulting from intrapelvic disease in women are:

I. Disturbances of surface temperature.

II. Neuralgia, such as spinal irritation, spinal ache, sciatica, migraine.

III. Special forms of headache, such as pressure on the vertex while the rest of the head is unaffected.

IV. Neuroses of the gastro-intestinal canal, including the familiar example of the persistent vomiting of early pregnancy.

V. Neurotic conditions of the breast, sometimes of the most aggravated character.

VI. Genito-reflex irritation of the respiratory tract, producing not only occasional dyspnœa but unmistakable attacks of asthma, of which Hegar, Grünewald, Engelmann, and Holden narrate examples.

VII. Hysterical affections of the joints and of organs of special sense, particularly of the eye, as is well known by oculists.

In the brief time at my disposal it would be as inappropriate as it would be impossible to attempt any minute subdivision or illustration of these general groups. But the reader who desires that will find the subject amply treated in an article on the "Hystero-Neuroses," written by Dr. George J. Engelmann, of St. Louis, and published

in the second volume of the "American System of Gynæcology."

I shall advert to only two points of practical importance.

First. It is futile to treat these illustrations of peripheral irritations as diseases. Like all neuroses, they are symptomatic only of some more deeply seated disease elsewhere, and only by combining the appropriate local treatment of that with improvement of the patient's general health can we hope to achieve success. I say "improvement of the general health" advisedly, for in all these cases there is feebleness of tone, often waste of tissue and marked hydræmia, and a general tendency to nerve exhaustion. The practitioner who fails to recognize this and who looks to the local treatment only, however intelligently he may conduct that, will be sadly disappointed in his results.

Second. It is a striking clinical fact that mal-conditions of the uterus exercise far more influence in this direction than disease of the tubes or ovaries, however pronounced the latter may be. Undoubtedly we often find an ovaritis, or the evidence of cystic or sclerotic degenerations of the ovaries, in these cases. And in like association we also find the various forms of salpingitis. But however thoroughly these are treated—whether by palliative means or by complete ablation—the neurotic complication will almost surely persist until the accompanying disease of the womb disappears. If there has been subinvolution, that must be overcome; if endometritis and uterine catarrh, perfect drainage from the uterine cavity must be effected and maintained; if displacement, reposition; if hypertrophy and endometritis, or chronic metritis with a lacerated cervix exist, trachelorrhaphy must be performed before the patient can be cured.

I have before stated, and in this Academy, that in neurotic conditions ablation of the uterine appendages is not only commonly useless, but often leaves the patient worse

than she was before. I repeat that statement now, as the result of much experience of my own and a not inconsiderable observation of the cases of others. That the appendages should be removed if intractable disease of structure affects them is unquestionable—and by this term I mean to designate serious pyo-salpinx or extensive ovarian degeneration; to assume any other ground than this would be absurd.

But, short of these conditions, they should be left where nature placed them; and, even when removed in the treatment of such conditions as are now under consideration, the accompanying uterine diseases must receive our most anxious care.

THE RELATION OF PERIPHERAL IRRITATION TO DISEASE; CONSIDERED FROM A THERAPEUTIC STANDPOINT.*

By SIMON BARUCH, M.D.,

PHYSICIAN TO MANHATTAN GENERAL HOSPITAL, AND TO THE NEW YORK JUVENILE ASYLUM, ETC.

THE most important element in the discussion of this question is the influence of its decision upon therapeutic procedures.

Whether peripheral irritations be etiological factors by reason of sympathetic effect, as was formerly taught, or, as a more refined pathology and more scientific inquiry into pathological processes claim to have ascertained, it be due to reflex agencies acting through the spinal cord, the chief aim of our therapeutic endeavors must be at the point of irritation. The removal of the source of peripheral irritation becomes imperative. If, on the other hand, as Dr. M. Allen Starr has correctly pointed out, and as the general practitioner but too often has occasion to observe after much bootless attacking of peripheral disturbing causes by specialists, many so-called neuroses are really but the manifestations of a slight or serious defect of control by the higher centres, due to their impaired nutrition and consequent impaired activity, the course of the medical attendant must be clear.

Who shall decide these different problems, which now more than ever press for solution?

The method of arranging a symposium on the great medical questions of the day must be of advantage. It

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brings out prominently the elements of each question as viewed from different standpoints, if the essayists are selected for the diversity as well as the honesty of their respective views.

It was a wise suggestion, too, of Dr. Jacobi to afford the general practitioner a hearing on the subject offered for discussion, for he stands, as it were, upon neutral ground, watching eagerly the friendly contests of his colleagues and co-laborers the specialists, and appropriating the wheat as they sift it from the abundant chaff by the threshing of contending views.

Therapeutic questions have ever possessed a fascination for me. In the course of the past thirty years it has not infrequently been my privilege to observe the budding forth of plausible theories, to watch them as they developed into lusty plants, only to see them blighted when the clear light of strict clinical test beat upon them.

When the lamented Hack called the attention of the profession to the attractive theory of reflex neuroses traceable to the nose, I was, although he offered us such positive clinical illustrations, somewhat reluctant in recognizing the validity of his claim. Some of the arguments which were brought against the idea of sensitive areas in the nose as the cause of reflex troubles of diverse and utterly incompatible characters, confirmed the therapeutic scepticism which is the natural outgrowth of advancing years, and the doubts thus aroused were somewhat strengthened by the enthusiasm of the author. But a case presented itself, shortly after reading Hack's original pamphlet, which enlisted my interest. A man aged twenty-four years was brought to me by Dr. L. Peiser, with the following history:

He had been complaining for one year of a "sticking pain on the right side of the neck" about the apex of the carotid triangle. This pain was so severe at times that it deprived him of sleep. He also suffered from a dry cough and, what annoyed him more, violent attacks of sneezing ten or twelve times in succession, recurring four

or five times a day. His acquaintances twitted him because he "sneezed like a cat." His appearance was worn and dull. The right lower turbinated covering was cavernous and doughy, and touching the septum which presented numerous sensitive spots. The lower turbinated on the left side also touched the septum and was sensitive but not doughy. The uvula was elongated. Posterior rhinoscopy revealed spongy hypertrophies on the lower turbinated. The right anterior turbinated swelling was thoroughly cauterized with the galvano-caustic point under cocaine. Ten days later he claimed an improvement; two sensitive spots on the septum, were now cauterized. Six days later pains in the neck had disappeared. Cough continuing, a portion of the elongated uvula was cut off and the left lower turbinated was cauterized. After two more cauterizations of sensitive spots on the septum, his nasal fossæ were clear and free. Sneezing, cough, and pain subsided.

The attacks of sneezing in this case were probably due to mechanical irritation, for, as Hack has shown, and as I have frequently observed, the "erectile" turbinated coverings are subject to enlargement and to diminution by mental and physical causes. The cough, too, was probably due to the elongated uvula. But the pain in the neck was probably reflex.

It is just as important to adopt local treatment whether the peripheral irritation produces symptoms through mechanical or through reflex channels. But it is well to differentiate these conditions for the sake of scientific accuracy, as was done in this case. The first positive case which removed my scepticism on the reflex influence of nasal peripheral irritation is a cure of true epilepsy, the only one which has ever occurred under my observation:

A. K., aged sixteen years, a robust boy, had been suffering from distinct attacks of *grand mal* since the summer of 1884.

The free use of bromide, by my friend the late Dr. Wm. Frothingham, diminished the number of attacks and finally gave immunity for one year. But they returned with greater frequency, and continued despite increased quantities of bromide. Dr. Frothingham having referred the

case to me on April 1st, 1886, it occurred to me that an enchondroma on the left side of the anterior portion of the septum, which filled the entire fossa in front, forcing the ala far beyond its normal line, might be a peripheral irritant bearing etiological relations to the epilepsy. After failure with the galvano-cautery as advised by Woakes, I removed the entire growth with a Bosworth saw. The bromides were continued until September, 1889. He had an attack a week after the operation, but since then he has been entirely exempt, although during the past seven months he has taken no bromides.

An immunity from attacks for four years may probably be regarded as a recovery from the disease. Similar cases are on record in recent literature, the most recent addition being six cases of epilepsy reported by Schneider.* These cases, cured, after the failure of active bromide treatment, by removal of pathological conditions in the nose, furnish interesting reading to those who are sceptical regarding the influence of peripheral irritation in disease.

A disease like epilepsy, in the presence of which we stand almost helpless, demands the utmost scrutiny of search for possible etiological factors.

There are a sufficient number of cases recorded by reliable observers to warrant us in adding the nose to the field of possible points of reflex irritation. Empirical though such treatment may be, and probably inexplicable upon strictly scientific principles, it behooves us to remove all pronounced abnormal conditions existing in the nose, *as an auxiliary therapeutic measure.*

With regard to asthma, migraine, trigeminal neuralgia, and minor nervous affections, the conditions are different.

The nose presents a field for thorough study of the question at issue, and it should be utilized to the fullest extent. Unlike the ovaries and urethra or other internal organs, we may subject it to tests which are impossible in these organs. It is not difficult to ascertain the presence

* Berliner klin. Wochenschrift, October 28th, 1889.

of sensitive areas in the nose, even if the patient's attention has not been directed to them by actual symptoms. If irritation by the probe produces the paroxysm of migraine, asthma, or neuralgia, we have evidence that can be obtained from no other organ. And if, in addition to this, we succeed in removing the attack, either artificially or spontaneously produced, by complete cocaine anæsthesia of the sensitive areas, the etiological connection is demonstrated beyond a doubt, and the treatment is clear. In a recent paper on asthma, Professor Dieulafoy, of Paris, recommended a cocaine application to the nose as the first step in the treatment. This is a wise suggestion, for if, as our *confrères* the rhinologists claim with some show of reason, nasal irritation may be the starting point of an asthmatic paroxysm, the latter must surely be abated by inhibiting the sensitiveness of these spots. But here an error may creep in. It will not suffice simply to brush the nose with a two-per-cent. solution of cocaine. A bulb-pointed syringe or atomizer with numerous fine openings, introduced just within the nostrils, would distribute half a drachm of a four-per-cent. solution of cocaine over the accessible portion of the nasal fossa. If the patient will press the bulb or piston of the syringe during expiration, hold his head downward and avoid swallowing the fluid, no possible harm can ensue, while complete anæsthesia of the entire nasal tract would be attained. If an attack of asthma or neuralgia be averted by such anæsthesia, the proof is clear. How many of our enthusiastic friends apply such a test or any other test in these cases? Except the *post hoc ergo propter hoc*—than which none is at times more misleading—we have in these tests a perfectly available method, which I trust will be more frequently resorted to, of discovering the actual connection between peripheral irritation and disease at a distance.

I am of the opinion that the so-called peripheral irritations should only be treated as etiological factors, if they manifest themselves to the patient.

This holds good, however, of all organs except the nose.

The latter is, as the epithelium covering its lining mucous membrane indicates, a respiratory as well as an olfactory organ. As an olfactory organ its function may be greatly disturbed or entirely lost without actual damage to the economy. Even as a respiratory organ its functions may be interfered with without much embarrassment, because the mouth furnishes a vicarious channel for the entrance of air; hence only when very positive stenosis exists does the system rebel and call the patient's attention to its presence by embarrassment of the respiration or other symptoms. Sensitive areas, swollen and cavernous turbinated coverings, polypi, etc., may exist within the nose, as the cases I have cited witness, without being sufficiently recognized by the patient to call the physician's attention to them. It therefore becomes our duty to search for these abnormal conditions in all those functional nervous disturbances which have been reported as possibly connected with nasal irritation. The tests I have referred to are then readily applied, and the treatment is rendered more effective and less difficult than in many other disturbances. A word of warning may, however, be not inopportune at this point. Operations on the nose are not so free from danger as they have often been represented by some enthusiastic specialists. I am personally cognizant of two cases in which deviations of the septum were removed by the saw; one with serious, the other with fatal results. The first, operated on while apparently in good health, by one of the most noted rhinologists of the present time, suffered from hæmorrhages and septic fever, which seemed to be the starting point of a state of cachexia lasting over a year, during which a malignant and fatal disease of the stomach developed. The second case, also in good health when operated on by one of our most active septum-sawers, whose skill cannot be impugned, awoke the night succeeding the operation with vomiting of blood and with epistaxis. He fell ill, and was attended for three weeks by no less than eleven physicians and surgeons. When I took charge of him, the gentleman who

had operated gave an obscure history in the patient of feeble heart, loss of appetite, and great apprehension of a fatal issue. I found the urine had been reduced below twenty ounces daily, without microscopic or other evidence of renal disease. An unfavorable prognosis was given. Dr. A. Jacobi, who had previously seen the patient, was called in consultation. The patient died thirty-six hours after I first saw him, simply from exhaustion due to loss of blood and subsequent inanition. A post-mortem discovered no trace of organic disease. There is no doubt in my mind that this death was directly due to the operation for deviated septum, which had been properly and skilfully done, *for relief of embarrassment to respiration.*

When you are told, by our friends the rhinologists, that the operation for deviated septum, if skilfully performed, is free from danger, bear in mind this testimony of a general practitioner, and let it not be done except for good cause.

In other organs the difficulty of discovering points of irritation is not so marked as in the nose, because interference with their functions becomes more or less burdensome, and in a large proportion of cases calls for remedy.

The eye, for instance, does not brook infringement upon its normal condition without protest—a protest which may or may not be heeded, according to the intelligence of the patient and his capacity for resisting encroachments upon normal functions. Here it is important to inquire cautiously for symptoms, lest by leading questions we be led astray. But here, too, the connections, if such exist, between peripheral irritation and the existing functional nervous disturbance may be traced, and it should be clearly traced ere the diagnosis is regarded as exact.

Surely nothing is easier for the general practitioner than the inquiry for eye strain in connection with, or independent of, the attacks whose causative relations he is endeavoring to fathom. And, if he is in doubt, nothing is easier than to refer such a case to his friend the ophthal-

mologist. This has been my constant practice since it first suggested itself in my own case, which may be of interest in this connection. Up to within nine years ago I was a martyr to migraine, suffering one or more attacks weekly, which disabled me from work. Although I was conscious of a difference in the refraction of my eyes, I was assured by two eminent oculists that the difficulty was a simple myopia affecting the right eye, with which it was not advisable to meddle. On coming to New York and taking a course of instruction on the eye and ear, I discovered a double astigmatism. Dr. R. H. Derby was kind enough to make a thorough examination of the refraction under homatropine, by which he discovered a high degree of myopic astigmatism in the right and a minor degree in the left eye. The correction of the error of refraction not only endowed me with perfect vision, but the agreeable discovery that the migraine was removed was an additional and unexpected result. Not a single attack has occurred since the glasses were adjusted.

If we do not succeed in relieving the patient of the functional nervous disease for which the ophthalmic examination was advised, we may at least be content that no damage has been inflicted by the correction of any error of refraction that may have caused distress from eye strain.

This is quite a different matter, however, from the mutilations which have been recently recommended by some of our *confrères*. The mournful record of these mutilated eyes and disappointed patients is indeed, as has been remarked by another, a sad chapter in the history of this subject. The fact that the novel view that "asthenopia," from which nervous diseases of all kinds are now said to result, "is chiefly dependent upon muscular insufficiencies, and is generally to be relieved by operations upon these muscles," is entertained and urged by only a very small number of men who stand alone in this country, and that this idea has not received the slightest recognition in Europe, where the opportunities for testing its correctness are so abundant, appeals to the general practitioner

as a cogent reason against its acceptance. The Neurological Society of this city has fairly weighed the arguments advanced by these innovators, and now Dr. Roosa has added one of those valuable practical observations, of which he is so admirable an adept, which must remove the question from the arena of discussion. He has shown us that, among over one hundred healthy persons, eighty-four per cent. presented a want of muscular equilibrium, so-called heterophoria, and that, moreover, a great deal depends, as has been well known since Graefe's time, upon uncorrected astigmatism. I was somewhat surprised that Dr. Roosa deemed the refutation of this novel and unsubstantiated theory a cause worthy of his steel. But he has offered us a clinching argument which must lay this all too lively ghost to everlasting rest.

From a therapeutic standpoint the eye as a source of peripheral irritation demands as careful and painstaking investigation as the nose. I am convinced that both promise satisfactory therapeutic results. But our efforts must not be indiscriminate. *Nil nocuisse* should be our motto so long as uncertainty haunts our course. Fortunately we have in these organs means for ascertaining positively the existence of points of peripheral irritation and of remedying them harmlessly. I plead for the more general adoption of these tests before active treatment is adopted.

The sexual organs as sources of peripheral irritation have been ably discussed from the gynæcological stand point.

From a therapeutic standpoint I desire simply to plead for the same principles that I have enunciated regarding the eye and nose. Here, however, we meet greater difficulties. The male genital organs are more accessible to investigation and treatment than those of the opposite sex. Alexander Peyer, our own Otis, and many others have studied this subject from the specialist's point of view. They have offered us clinical data which cannot be controverted. But let us proceed cautiously, and always

bear in mind the motto, "*Nil nocuisse.*" Mutilation of these important organs must be avoided, unless the existence of a point of peripheral irritation is clearly established. We have no tests here, as we have in the nose and eye, so every other means must be exhausted before local surgical treatment is resorted to. Another point of therapeutic importance is to be remembered, viz., the danger of directing the patient's attention to the sexual organs as a possible cause of his functional troubles. Such a course is almost certain to increase the latter.

The existence of peripheral irritation in the utero-ovarian system has long been a vexed question. While I am convinced that a lacerated cervix is frequently an etiological factor of pronounced type, and while I regard the removal of the local pathological conditions connected with the latter as a *sine qua non* to the improvement of the health of many suffering women, I am also convinced that these lesions rarely if ever give rise to the reflex functional nervous troubles that have been attributed to them. The latter may almost invariably be traced to conditions of general ill-health and anæmia resulting from the local processes which give rise to muco-purulent discharges, to infection from raw surfaces on the cervix, and to interference by pain with comfortable locomotion, rather than to the pressure of cicatricial plugs, etc. These views I have fully set forth in a paper on the therapeutic significance of the cervical follicles,* and I must now be content with this simple reference to a subject which has given rise to more acrimonious and bootless discussion than even the insufficiencies of the ocular muscles.

The idea of the connection of ovarian irritation with functional nervous diseases is coeval with the history of medicine. Since gynæcology has become a full-fledged offspring of medicine this question has persistently clamored for solution. There has been so wide a schism in the ranks of the special worker in this branch—a schism

* New York Medical Journal, June and July, 1885.

which is happily narrowing to a more conservative point in recent years—that a general practitioner may well be excused from expressing his views, except to urge respectfully the warning, "*Nil nocuisse.*" I have searched the literature of this subject industriously for the clinical proof that the removal of the uterine appendages has been instrumental in removing pronounced functional nervous diseases. While I am convinced that the ablation of *diseased* ovaries and tubes must and does contribute to the improvement of health in some women by removing foci of infection, distress, and pain, and by removing a barrier to exercise and to the pursuit of the duties of life, I cannot bring myself to the belief that the removal of ovaries or tubes not presenting palpable and well-defined pathological changes is ever called for; and I deem such a procedure an outrageous violation of the highest aims of our calling, which demands the condemnation of every decent physician.

There are many other points of peripheral irritation whose relation to disease demands discussion. As one speaker has referred somewhat contemptuously to peripheral irritation as productive of nervous diseases, it may be well to mention that we have the authority of Professor Thomsen, of Berlin, for the statement that even serious psychoses may be traced to peripheral irritation. In the *Charité Annalen* for 1888 he gives the histories of five cases of reflex psychoses resulting from wounds of the head, with concentric narrowing of the field of vision and unilateral hallucinations; and two other cases in which scars in the periphery produced epileptic attacks, and later also psychoses, which were cured by excision of the scars. All these patients suffered anæsthesia or hyperæsthesia; and in all, the extent of the field of vision was an index to the amount of the psychical disturbance.

The relation of peripheral irritations existing in the gastro-intestinal tract to diseases elsewhere, has been recognized from the earliest time of medicine as an empirical observation by which the harmful practice of

indiscriminate purging has been upheld. Our literature abounds in well-defined clinical evidence upon this subject. Among the most recent and interesting observations I find one which, in this day of over-zealous specialism, may serve as an illustration of scientific conservatism. Prof. Gussenbauer has published* twenty-eight cases of intermittent trigeminal neuralgia which were referred to him for surgical treatment, and among which he found only four that required surgical interference. The remaining twenty-four yielded to careful attention to existing intestinal torpor. Some of our enthusiastic specialists would do well to emulate the example of this conservative surgeon, and ponder well and long before they inflict lasting deformities upon the patients committed to their care.

Nothnagel has clearly shown in his Klinik für Darmkrankheiten that intestinal activity depends upon the functionating capacity of the intestinal glands, and that many cases of constipation may be traced to the diminution of automatic action in the nervous apparatus of the intestines. That pathological changes in the ganglia of the intestinal muscles, producing constipation, may convey nervous impulses through the sympathetic to the cerebro-spinal system, and then produce abnormal sensations, has been often practically demonstrated.

Nothnagel believes that hemicrania from constipation is due to a venous stasis in the intestines, which impairs the functions of the latter by reason of an insufficient supply of oxygen, so that intestinal irritability or automatic activity is diminished. The latter produces abnormal excitation of the cerebro-spinal system, resulting in an irritation or paresis of the vaso-constrictor fibres of the cervical sympathetic, which in turn produces the vaso-motor phenomena of hemicrania.

Sciatica, formerly supposed to be due to pressure from fæcal masses in the sigmoid flexure, may be similarly explained. This brief reference is made in order to illus-

* Prager medicinische Wochenschrift, No. 3, 1886.

trate that many functional nervous conditions may be removed by the old-fashioned and rationally explicable treatment of constipation, and to emphasize thereby the lesson that careful and industrious search for foci of peripheral irritation should be made *in every direction* before the eyes and nose, the urethra, or, worst of all, the uterus and ovaries, are attacked and mutilated.

In conclusion, permit a general practitioner who has endeavored by many years' service in the eye, ear, throat, and gynæcological departments of our city dispensaries, and by industrious attendance upon the clinics of our metropolitan hospitals, to keep himself abreast of the advances of special departments in medicine, to sum up his views on the therapeutic significance of peripheral irritations as follows:

1. The existence of peripheral irritation as an etiological factor is well established.

2. There need be no conjecture in the search for such causes of functional nervous diseases in many cases, because we have means, in at least the more recently discovered sources of peripheral irritation, the eye and nose, of detecting and testing their existence.

3. All harmless methods of treatment should be exhausted before mutilating procedures are adopted.

4. Whenever there is a doubt the local condition should receive the benefit of that doubt, and treatment should be directed to the improvement of the general health, the reestablishment of defective secretions, and to the removal of the patient from unfavorable environment.

THE RELATION OF DENTAL DISEASE TO PERIPHERAL IRRITATION.

By M. L. RHEIN, M.D.

Not prepared for publication.

THE AUSCULTATORY PERCUSSOR.*

By L. L. SEAMAN, M.D., LL.B.,

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In the furtherance of a profession which has long since broken away from the traditional empiricism of the past in its observance and devotion to both science and philosophy, the development of any new mechanical appliance for the perfection of diagnostic investigation requires neither apology nor justification. It is enough that there is a work or service yet inviting professional research, and a sufficient provision for it, in the world of discovery and invention. Indeed, this absolute *entente cordiale* between the physician's art and science, both speculative and practical, is a matter of supreme moment as well as supreme satisfaction. Correct and exact knowledge of morbid conditions is indispensable to the practice of our profession, and in no department of physical diagnosis has the practitioner encountered more critical and perplexing difficulties than in the exercise of percussion and auscultation.

The auscultatory percussor is an apparatus constructed on strictly scientific principles, by which the practitioner is enabled to realize and delicately discriminate the percussion note, while adding to ordinary percussion the value of mediate auscultation, thus securing a trustworthy and explicit tonal reading of internal conditions. In conjunction with the Edison phonograph it also furnishes an instrument whereby percussion notes may be accurately registered.

I submit the instrument and its mechanical appliances,

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* Read before The New York Academy of Medicine, May 15th, 1890.

together with an analysis of the acoustical laws under which its researches are utilized. In further elucidation of the scientific data represented in this invention, it may be observed that all forms of matter capable of resonant vibration respond in characteristic and fundamental notes. For, says the "Encyclopædia of the Art of Acoustics," "the shortest and most sudden noise has its peculiar character." But it must be borne in mind that these vibrations in passing from one medium into another become modified, if not changed, in character, after the analogy of transmitted light or radiant heat; and any variation in the media through which they pass causes a correlative variation in the character of the vibration itself. On this single fundamental fact or principle of tonal or resonance development the science of diagnostic percussion depends. In the percussion of a healthy thorax a sound is developed which in degree and quality of sonorousness is accepted as a normal standard. This standard, however, is not uniform in different individuals, but varies according to the varying thickness and elasticity of the chest walls, and to other attendant physiological conditions, the normal standard for any individual being determined by comparative percussion of both lungs, or of one area of a lung with the corresponding area of the other. It is obvious that pathological conditions occasioning derangement or disturbance of legitimate functions give rise to changes or modifications in the note of percussion which the practitioner often interprets with great difficulty. Deviations of resonance in the incipient stages of disease are exceedingly subtle and are identified with extreme difficulty at the very stage when their full significance is of paramount importance. This instrument has been devised especially to secure a more truthful reading of these obscure variations in resonance, and thus to facilitate the examiner in arriving at a more exact diagnosis.

It will not be disputed, after reflection, that all sonorous vibrations falling within the range of our perceptions

have their exterior correlatives as normal conditions of the surrounding atmosphere. To illustrate, if the air could become actually visible as it generates and continues the sound in response to a stroke of the percussor, we could clearly distinguish and identify those conditions of notion on which the sound depends; while its quality, for the moment, would be determined as a condition or state of the volume of atmosphere under examination.

Tyndall graphically compares the striking of a cannon-ball on a target to an atmospheric particle striking the tympanic membrane of the ear. "The sonorous effect is expressed by the same law as the mechanical effect." "All, in fact," he remarks, "that goes on outside ourselves is reducible to pure mechanics; and if we hear one sound louder than another, it is because our ears are hit harder in one case than in the other."

Therefore, in striking a blow we set in motion a volume of matter which augments as the square of the distance from the starting point, while the intensity or loudness of the sound thus produced diminishes in the same ratio, or inversely as the square of the distance. Thus it becomes evident that if a mechanical impulse be communicated to any particle of air, and thence to the surrounding atmosphere, and permitted to radiate without interference, it must reach each more remote point with an energy which is constantly diminishing under a uniform ratio; for the reason that the original energy of impulse is communicated to a constantly increasing volume of atmosphere at rest.

The propagation of sound is governed by the same laws of reflection observable in the transmission of light; the angle of incidence and of reflection being found equal under both acoustical and optical analysis. Divergent waves of resonance, therefore, may be duly gathered, concentrated, and propagated through the intermediary of properly adjusted reflecting walls, and delivered, unimpaired, with tonal truthfulness, at a desired aural focus in obedience to well-known, clearly demonstrable laws

The manipulation of the instrument invites close attention. The object end is placed directly over the area under examination. The thumb raises the lever, and when at a given height it is released, the action of the spring throws the hammer vigorously down, delivering a blow on the pleximeter. The impulse thus imparted forces the air into sonorous undulations which, after deflection along the walls of the receiving chamber, are propagated without dispersion directly to the ears of the listener, be it observed, in isochronous delivery to both ears, thus securing the most perfect tonal report conceivable to scientific speculation.

The receiver does more than merely reflect and transmit the sonorous undulations. Since it is in immediate contact with the vibrating chest walls, it takes on reciprocal resonance, which it communicates to the observer, thus intensifying the report. Thus the vibrations, instead of being suffered to radiate in every direction, are collected and delivered convergently to the auditory nerves; the lesser deviations, whether of pitch, duration, or quality, are sufficiently augmented to render variations from the normal standard immediately apparent.

Another distinct value of the instrument is that the force of the blow delivered may be graduated and regulated to the finest degree of nicety by a proper adjustment of the set screw, varying areas may be examined and comparative results deduced with almost absolute precision.

Again, the stroke of the hammer is always exactly perpendicular to the pleximeter. Walsh, in his chapter on "Physical Diagnosis," in speaking on this point says: "The least variation in this respect will assuredly be attended with a difference in the sound elicited." It is apparent that the quality of the resonance must vary with the direction as well as the intensity of the stroke. The least departure, then, of the percussory blow from the fixed perpendicular vitiates the accuracy of the examination. Nothing is clearer than the fact that the ordinary

digital stroke is subject to perpetual variation, even under the exercise of the most intelligent precaution.

The shaft on which the percussor is fastened is so adjusted that the spring, while expending its force, throws the hammer a little beyond the line of centre, so that the blow is struck lightly and quickly, and the hammer rebounds, very much as it strikes and rebounds from the string of a piano when a note is struck on the keyboard. By this means the pressure is removed the instant after the blow is delivered, so that the vibrations of the area under examination are neither interrupted nor impeded. The pressure of the pleximeter on the chest is also constant, and this, too, is an important consideration; for, says Walsh in the article just referred to, "no extrinsic condition modifies sound so much as the amount of force with which the pleximeter is applied."

By an additional object end, the instrument may be converted into a flexible stethoscope, identical with Dr. Cammann's. This instrument, according to Dr. Flint, so intensifies sound that auscultation is rendered available, even to practitioners with impaired hearing. By the intermediary of the auscultatory percussor, diagnostic percussion is also made practicable for this unfortunate class of the medical profession.

While explaining the practical operation and use of the instrument, stress was laid upon the fact that the report of resonance is delivered simultaneously to both ears of the examiner.

In further elucidation of this point, we refer to another law of acoustics, in obedience to which auditory sensations from a given sound are fuller and more perfectly expressed when delivered to both ears. This law is illustrated analogically in the use of the binocular field glass and binocular microscope. This instrument is, then, a binaural auscultatory percussor in the isochronous conveyance of its tonal report to both ears of the listener—thus serving while complementing the binauricular con-

stitution of man himself, in his dual function of hearing as well as seeing.

When it is desired to register the percussion note, the binaural tubes of the instrument are replaced by a single one which is connected with the receiving funnel of the phonograph. The percussion blow is struck as before, and the undulations of sound are communicated directly to the diaphragm of the phonograph. This delicate mechanism registers the notes upon its wax cylinder, as illustrated on the specimens herewith submitted. These cylinders may be stored for an indefinite period, and the sound reproduced at will on the phonograph, with sufficient force to demonstrate the original notes.* With the assistance of the microphone these notes may be distinctly heard by a large audience.

When it is remembered that the "most delicate shades and variations of the human voice—music, whether instrumental or vocal, solo or multiple, in all its rhythm, melody, and intonation, the lowest as well as the highest notes, may be treasured up and reproduced, duplicated and multiplied indefinitely," it may readily be seen how the phonograph, aided by the auscultatory percussor, may be destined to play an important rôle in the medical teachings of the future.

There are objections to be anticipated and apprehended, as under all similar introductions of scientific and mechanical appliances in the furtherance of our profession. It may be urged that the investigator is liable to suffer confusion and disturbance of auditory impressions, since the instrument intensifies and augments resonances, which might prove misleading. Undoubtedly such objections would be formidable if the explorations were confined to but a single area of the chest; but they lose their force when it is remembered that the investigator will differentiate his conclusions from a delicate comparison over

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* At the conclusion of the paper the notes of the cylinder were reproduced on the phonograph.

several chest areas; so that local dulnesses can be sharply marked out and intelligently interpreted. It is obvious that the profitable employment of such an intermediary, or of any untried mechanical or scientific invention, implies manual and technical dexterity and well-practised, intelligent manipulation, and especially a careful training of the hearing in the study and interpretation of resonances, before practical results may be secured.

Many of the objections so forcibly urged against the Cammann stethoscope may without doubt be ventured against the auscultatory percussor, such as "noises in the ears" produced by the pressure and presence of the knobs, resonance of the receiver, etc.; but as Dr. Allison has already so admirably answered them, they may be dismissed without further notice. Yet some good purpose may be served if I quote his words: "It is common to hear persons speak with disparagement against this instrument (the Cammann stethoscope) on account of the little evils above pointed out. But it would be better for such individuals to master a valuable aid than to carelessly and heedlessly throw it aside and prevent others from using it. All instruments require education and care; even the wooden stethoscope in the hands of a clumsy explorer may fill his mind with prodigious errors. In a difficult, in an obscure pursuit, in dealing with the lives of those confiding themselves to our care, we are imperatively called upon to think seriously, and to try, time after time, before we fling away any contrivance promising assistance in ever so slight a degree. To use skilfully a delicate instrument requires time and application, and this is no more objection than to the acquisition of a language."

SPINAL SURGERY—A REPORT OF EIGHT CASES.*

By ROBERT ABBE, M.D.,

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I HAVE the pleasure of presenting this evening, not a rose-tinted picture of startling achievements of new surgery, but rather a serious review of some of the grave cases of spinal troubles requiring surgical interference which have of late come under my care. They had all been subjected to prolonged medical treatment, and were, when referred to me by the physicians or neurologists, in nearly a hopeless condition. Almost as many more cases have been refused operation, but these seemed either to offer hope of relief based on experience, or to give legitimate opportunity for novel operative methods based on sound physiological principle.

The popular idea among physicians has been that the spinal cord is more inaccessible to the surgeon than the brain, by reason of its irregular bony coverings and the hæmorrhage from the venous plexuses that envelop it, and that injuries and diseases of it are to be looked upon hopelessly unless Nature kindly assumes to work unexpected recoveries. This view is shared even by the public.

It is from the ranks of these doomed cases of paralysis of the lower half of the body, and some other spinal troubles, that an effort is being made to cull out some cases which, heretofore neglected, may yield good results.

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* A paper read before The New York Academy of Medicine, May 15th, 1890.

A few noteworthy articles have appeared during the past two years reporting operative cases. Those by Horsley, MacEwen, Thorburn, White, Bullard and Burrell, Winslow, Dandridge, and Bennett are of special interest.

It is evident that some cases of improvement have been reported prematurely, and it is fair to ask some months' delay before a published report.

Especial interest has been aroused by the brochure of William Thorburn, of Manchester, just issued, giving, I believe, the most thorough and dispassionate review of this field that has yet been made. While realizing that his most lucid views are based on the study of all cases reported up to the present, one is struck by the fact that these are comparatively few. The need of reporting carefully all cases where operation is done leads me, therefore, to present the following eight cases, which may be divided into four groups:

Three of paraplegia from fracture. One of early curetting of a vertebra for Pott's disease. Two of tumors of the vertebral canal with paraplegia. Two of intradural section of some of the posterior roots of the brachial plexus for neuralgia.

I will read the three fractural paraplegia cases together. The first one was:

CASE I. *Fracture of the Spine between the Eleventh and Twelfth Dorsal Vertebræ; Complete Paraplegia and Anæsthesia with Incontinence; Operation, Eleven Months Afterward, Breaking up Intradural Adhesions; Suture of Dura; Primary Union; no Relief of Paralysis up to One Year Afterward.*—R. M. G——, aged twenty-seven, merchant; a gentleman above the average height and in good health until May 19th, 1888, when he fell from a platform twenty-one feet high, while pushing off a large beam. Fell with the timber on sawdust-covered ground, receiving scalp wounds, but it is impossible to say whether the timber struck him or not. He was unconscious for three hours, and was completely paralyzed and insensitive below his waist when he recovered. Efforts were made,

under ether, to straighten the fracture deformity of the spine by extension and manipulation, but nothing was gained. A bed-sore formed over the deformity, and a water-bed was obtained. At first it was filled with very cold spring water, and he suffered such pain on being placed upon it that he fainted and was unconscious for hours. Complete incontinence of urine occurred from the first, and his bladder was irrigated for cystitis. Diarrhœa was succeeded later by complete inaction of the rectum, requiring the persistent use of laxatives.

After three months he resorted to a wheel chair and attended to business. This was his condition in April, when he was brought from Texas to Dr. Lewis A. Sayre, who referred him to me.

He was in good general condition, though having twelve per cent. of albumin in his urine, which decreased to seven per cent. before I operated. The line of anæsthesia was sharply defined, and above it no hyperæsthesia.

April 12th, 1889, I operated with the assistance of Dr. Weir. The method pursued being typical, I give it in detail, and will not repeat it for the other cases. The back was shampooed the evening before, and a damp sublimate dressing kept applied until the moment of operation.

The patient was laid prone, but with one shoulder raised by a sand pillow—favoring easier respiration and inclining the back toward the operator slightly. A free incision was made parallel to the spines, and a half inch to one side, cutting the longissimi attachments from one side only, and being carried clean down to the laminæ at the second or third pass of the knife. To approach the fracture between the eleventh and twelfth, the incision was made from the eighth dorsal to the first lumbar spine; the laminæ were then cleared of muscles, which were drawn outward by retractors, and the ligament divided above the spine of the eighth and below the eleventh, thus isolating a block of four spines, whose bases were then severed from their arches by stout cutting pliers.

This manœuvre at once allowed a retraction of the entire block of connected spines with their muscles still attached on one side, and the entire breadth of the spinal arch was thus exposed without sacrificing the overlying tissues. A pair of slightly curved rongeurs was then applied to the lower edge of one lamina, and with in-

credible ease the entire breadth was quickly gnawed away. Three arches, the tenth, eleventh, and twelfth, were thus treated, and the clean spinal cord laid bare for two and one-half inches. The twelfth dorsal vertebra was found to have been displaced backward, the fracture running through the articular facets, the pedicles and laminæ. The cord was compressed between the arch of the eleventh above and the upper lip of the body of the twelfth below. The intervertebral cartilage had been ruptured. Above the line of pressure the cord pulsated, below it did not.

In half a minute after the cord was released from its flattened state (the bone pressure area being only half an inch deep) the dura became rounded up as full as it was above or below. When it was filled out there was no surface appearance by which one might say the cord within was not normal, except that the upper portion pulsated. I emphasize this because surgeons so often have reported that the cord appeared normal, therefore they did not open the dura. The wound now being irrigated and dried, the dura was slit up for two inches. Adhesions of various densities were found within, attaching the meninges to the dura. They formed a complete circular dam shutting off the upper from the lower part of the canal. Only an ounce of clear spinal fluid came out, as the spinal canal had purposely been inclined on the table so as to slope with the head lower. The veins of the cord were not distended. The adhesions were, with very little force, broken up.

The cord was normal in thickness above the involved part; then, by a sloping rather than abrupt change, it merged into a flattened cord for three-fourths of an inch, retaining its breadth but less than half its thickness, the principal atrophy seeming to be in the posterior columns. Throughout this flattened portion the white fasciculi of the cord could be traced continuously, so that there was no abrupt break in its continuity.

Before operating it had been proposed to the patient—whose unusual intelligence and thorough appreciation of his hopeless state enabled me to lay all the possibilities of spinal surgery up to that time before him—that if we found the cord destroyed within narrow limits and apparently sound above and below, we might excise the destroyed portion and suture the fresh-cut ends, if in our judgment it seemed wise. Though told it had never been done, he accepted the experiment. It proved to be an

impossible operation, however, in this case. To judge whether it would ever be possible, I tried to approximate the sound cord on either side of the damaged part by traction made with tenacula embedded in the meshes of the membranes at such points as would have been available for sutures.

There is a slight latitude of motion of the cord vertically in the canal, and I judged at this trial that I might have excised a scant quarter of an inch, and then approximated the ends by sutures that would not tear out. The damaged cord in this case was three times that length. No further repair being possible, the dura was sutured by fine catgut. Then the displaced spines were brought into line and sutured by heavy catgut to their neighbors above and below.

The fascia investing the muscles then received two or three interrupted catgut sutures with gaps for drainage, and finally the skin was drawn partly together by a few catgut sutures not tied, but left for use at the next dressing. No drainage tube was applied, but a piece of protective three inches wide laid over the wound, the skin edges being left a quarter of an inch apart so as to allow drainage from the deep portion. The investing antiseptic dressing was covered by a plaster jacket covering only the back, like a turtle shell, and secured by an enveloping canton-flannel binder pinned in front.

In forty-eight hours the dressing was changed.

Drainage had been perfect; the wound had healed except the skin, which the sutures, already in place, now brought together when tied, and a final dressing was put on.

From the time of operation he had no pain in the back or extremities. The wound healed primarily, leaving only a linear scar. He remained three weeks in St. Luke's Hospital, with no fever or other disturbance. His condition has been watched since then, and there has been no improvement in motion or sensation.

For six weeks he wrote encouragingly, and claimed that he could draw up his toes an inch and had returning sensations. A recent letter, however, written nearly one year after operation, admits that he has gained nothing. Yet in general flesh and mental welfare he has never been better, weighing more than ever, and enjoying life from his perambulator which he wheels about.

About six weeks after operation he passed through a curious two months' illness of the nature of trance. He

writes me that he began by having severe pain in his feet and legs every morning, then became unconscious after two weeks and remained so, yet seeming to be in pain while thus sleeping. Soon he became calm and slept constantly, sometimes answering questions, and sometimes not understanding; but he seemed to know who was about him, and would usually tell. He came out of this abruptly, and was in every way mentally himself again.

I judge this to have been an effect left by reaction after two months of exalted excitement and harboring the "exhilarating sentiment of hope," followed by swift appreciation of the unchanged paralysis, and that it is only one of the curious hysterical manifestations occasionally connected with spinal disturbances.

CASE II. *Fracture; Paraplegia below Eleventh Dorsal, Two and One-half Years' Duration; Operation.*—G. W. I——, aged twenty-seven, merchant. In October, 1886, the patient was thrown from his horse on his ranch in Montana, and struck his back across a stick on the ground, injuring the spine at the junction of the dorsal and lumbar vertebræ. Instantaneous and complete paraplegia resulted below the waist, with paralysis of the rectum and bladder. He lay where he fell for a day and a half before he was discovered, during which time he was exposed to sleet and snow. When he was found he was taken to a farmhouse and the nearest physician was summoned from a distance of ninety miles. During his exposure of a day and a half he had no food or drink. He had an overdistended bladder, but before the doctor's arrival he devised a method of his own for relieving his bladder by means of straws, the ends of which were rendered smooth by dipping in candle-wax. As a restorative after arriving at the farmhouse, bottles of hot water were applied to his feet—so hot as to cause blistering, which resulted in gangrene. After three weeks he was removed to his home in Connecticut, where it was found necessary to amputate both legs below the knees. The bladder was catheterized for a week after the accident, but subsequently emptied itself when full without his control or knowledge. Rectal movements were also involuntary. Subsequently he suffered from severe sacral bed-sores. The paralyzed limbs often had a sense of burning and severe pain. Bending the knee forcibly caused pain, and

at the time the legs were amputated, no anæsthetic being supposed necessary, the sawing through the bones caused intense pain.

His general health recovered so that he was able to be placed in his carriage and driven about. Indeed, he was constantly in the habit himself of driving, of which he was very fond, being secured in the wagon seat by straps.

A few months before he came under my care, which was two and one-half years after the accident, he had unwisely remained in his buggy about seven hours. His anæsthesia rendered him unconscious of discomfort from prolonged pressure, and there resulted a large area of pressure gangrene under each buttock. The resulting pressure sores refused to heal, and thereafter he was confined to his back. His condition became so desperate that life had little in it for him, and he had even threatened to shoot himself if left alone. With the hope that some method of operation might be devised for his improvement, he was brought to me at the Post-Graduate Hospital by Dr. Smith, of Meriden, Conn. Examination showed absolute paraplegia and anæsthesia below the line crossing the sacrum at its upper border, and extending in front across the abdomen two inches below the navel. The bed-sores under the buttocks were unhealed, exposing the bone. There was involuntary muscular jumping in both legs. Urine showed granular casts, but no albumin. The patient had strong and natural erections with emissions.

He was examined by Dr. Dana, whose conclusion was that the cord was absolutely severed at the last dorsal, but that below the second lumbar it must be in fairly healthy condition.

The case seemed absolutely hopeless, unless it were possible to innervate the lower segment of the cord by renewing its contact with the upper, or, what seemed not impossible and perhaps worthy of trial, to carry out a suggestion of Dr. Dana's, in case the parts were not too much injured, "to cut off a few of the lower dorsal roots long, and of the lumbar roots short, and suture them together, thus increasing the chance of getting sensation."

The man's hopelessly bed-ridden and suffering condition led him to urge us to any operation, however novel or dangerous, that offered the slightest chance of improvement. I therefore operated, April 18th, 1889, at the Post-Graduate Hospital, by the same method as detailed in former cases. An incision was made from the ninth dor-

sal to the third lumbar vertebræ, and the spines of the tenth, eleventh, and twelfth dorsal and first lumbar were clipped from their arches and drawn aside with attached muscles, exposing the entire spinous arch. It was then found that a massive and dense eburnated deposit of bone had formed at the site of the eleventh and twelfth dorsal and first lumbar laminæ. This was with difficulty chiselled and cut away until the severed end of the spinal cord was exposed and found to be completely cut across and the dura sealed up. Below this, for one and one-half inches, solid bone filled the vertebral canal. The cord here commenced again, and its end was found engaged in the bone so that spiculæ had grown into it. It could not be lifted up to approach the upper end more than half an inch, and it was evident repair by suturing was hopeless. The operation had been unusually bloody on account of numerous venous sinuses which were opened in the bone, and the patient was considerably exsanguinated. He rallied fairly well from the operation by thorough stimulation and saline infusion of thirty ounces into the vein of his arm, but died thirty hours after, the kidneys secreting only four ounces of urine during this time.

CASE III. *Fractured Eleventh Dorsal Vertebra; Paraplegia; Operation; Opening Dura; Recovery Unimproved.*—J. S——, aged twenty-seven, coachman. On January 1st, 1889, alighting from the rear platform of a "bobtail" car while going rapidly, he was struck in the small of the back by the platform, fell on his hands and knees, suffered acute pain in his back, and was paralyzed at once. He was moved in an ambulance to a hospital, and plaster jacket applied for a month. Had incontinence of urine and fæces. His urine was acid and contained a trace of albumin. Had no pain in his legs, and paraplegia was complete. Admitted that he could feel when his legs were touched, and complained of tingling in both feet and legs. On examination a slight depression was seen between the last dorsal and the first lumbar spines. A line of anæsthesia crossed the back at the top of the sacrum. At the sides it ran one inch above the crest of the ilium and crossed the abdomen two inches above the pubes, the line following the curvature of the groin along Poupart's ligament. There was atrophy of all the muscles of the thighs and legs. Complete paraplegia and anæsthesia, the skin of the lower extremities showing persistent vaso-motor impressions.

February 28th, 1889, operation under ether. Incision seven inches long from the ninth dorsal to the second lumbar. Operation essentially the same as in the case narrated last. The laminæ of the tenth, eleventh, and twelfth dorsal and first lumbar were removed by rongeurs. The arch of the eleventh dorsal showed evidence of recent fracture, but there was no displacement and no pressure. The dura appeared evenly full and round, and, as far as its surface showed, nothing would be suspected wrong within it. It was, however, then slit for three inches and a half; from the lower part a little fluid escaped. At a point underneath the injured arch a circular dam of lymph was found, one-eighth of an inch wide, between the dura and the cord, entirely shutting off the upper from the lower part of the canal. From above this dam the arachnoid fluid flowed freely. The cord at this point showed evidence of having been completely crushed, the white substance ending abruptly and beginning again something over half an inch below. In the intermediate space the cord was represented by a pinkish-gray substance, apparently consisting of only the meshes of the membranes, but no white fasciculi. Just below the lymph dam a mass of largely distended veins occupied the surface of the cord, showing obstructed venous return. These entirely emptied themselves upward when the pressure was taken off and the lymph parted by the probe. The cord was entirely liberated from its adhesions, the dura sutured with fine catgut, and the wound closed.

The patient convalesced without fever, the temperature never rose above 99°, and the wound healed promptly with no escape of cerebro-spinal fluid. On the following day I was surprised to find that the patient had acute hyperæsthesia of all the paralyzed part. A touch or pulling of the hair on the feet, legs, and thighs instantly gave acute pain and involuntary jumping of the limb. There was absolutely no voluntary motion, however. He was able to identify the part touched, as witnessed by Dr. Dana and myself and the house staff.

During the second week this hyperæsthesia abated, and he was as he had been before. The muscular response to faradism was improved, but there was no volitional action up to six weeks after operation.

All operators upon cases of fracture paraplegia of any duration have thus far arrived at about the same conclu-

sion—namely, that the pressure of bone is of the most secondary importance, except the fracture involves only the arch, where it is driven in by a blow, inasmuch as the violence, usually a fall and bending of the back, which will produce instant paralysis, has done so by a diastasis of the vertebræ, the cartilage being ruptured and the arches broken, which completely pulpifies the spinal medulla. The vertebræ are very apt to immediately resume their usual relations. If, however, the fracture takes place at or below the last dorsal, where the medulla has disappeared and the firm cauda equina commences, the crushing does not usually destroy the nerves, but long bone pressure would. In such cases operation to correct it is always desirable. It still remains a problem, perhaps never to be solved, how to connect the lower segment of the cord with the upper when there is a gap of half an inch, and whether this union would restore functional connection with the brain, even though its reflex and independent activity may be ever so good. The cases may yet be found where sufficiently narrow transverse lesions will allow suturing fresh-cut ends of the cord.

In other cases it remains yet to try the suturing of nerve roots from above the break into some one or two below, sacrificing only the cutaneous supply for a small area, as suggested by Dr. Dana, or, as seems feasible, implanting them into the cut end of the lower stump, thus perhaps innervating some limited portion of the distal end, with the chance of stimulating more. Possibly in this way even the vesical control alone might be restored. This is conjecture entirely, and based on the fact that clean-cut nerve sections will unite.

The next case is one of Pott's disease, taken early and treated as a tubercular caries in any joint would be. The only points of interest are indicated in the report.

Case IV.—Eugene K——, aged twenty; glass worker. This case is narrated simply to show the ease with which the vertebral bodies, if carious, may be approached from behind.

Two years before admission the man had had a pleurisy from which he recovered. Subsequently he was cured of a fistula in ano, and when he came into St. Luke's Hospital he had slight phthisical changes in the apex of the left lung, and a lumbar abscess prominent over the iliac crest. The latter was opened in September, 1889, by Dr. Curtis, and discharged profusely through three sinuses around the crest of the ilium.

In February last I found a long probe passed upward to the last dorsal vertebra, and, as there was no deformity and but little pain, I considered the possibility of curetting the carious bone.

On February 5th I incised beside the twelfth dorsal, guided by the end of the probe. The transverse process of the twelfth was carious and was cleared away with a bone curette. This instrument was then worked into the body of the bone alongside the spinal dura, without injury to the latter, and a large excavation of softened bone removed to the extent of about half of one vertebral body, when on every side the curette encountered firm and apparently sound bone; the entire course of the pus track through the soft parts was curetted, and douched with sublimate solution and finally with solution of iodoform in ether. The wounds were dressed with the usual care. In six weeks the patient was sent from the hospital with only a slight discharge, and with but one sinus.

The course of this case shows that where there are long sinuses connected with a small carious bone focus the great proportion of purulent secretion is from the sinus walls, uniformly lined with tubercular granulation. It further illustrates the ease with which the excavation and drainage can be accomplished directly backward through one side of the vertebral canal, by pressing the uninjured dura aside.

I now speak of two cases of unusual interest, because they represent a field in which the extreme nicety of diagnosis does credit to the physicians in charge, and surgical relief follows closely.

CASE V. *Extradural Tubercular Tumor of the Spine; Paraplegia Complete; Operation; Recovery.*—I epitomize the narrative of this case, which has already been reported

in full at the State Medical Society in Albany one year ago (New York *Medical Journal*, February 24th, 1889).

G. P——, aged twenty-two, was taken with pain in his back in January, 1888; came to St. Luke's Hospital in March. The spine was flexible and without deformity, though a very slight fulness was seen in the soft parts to the right of the ninth and tenth dorsal spines. During March sense of touch was dull in his legs and the muscular power somewhat weakened. A line of hyperæsthesia formed about his waist. Two weeks later he could not stand without support, and he had uncontrollable twitching of the legs, which had become quite anæsthetic. He had constant intercostal pain, with girdle pains about the limiting line of disease. Incontinence of urine and fæces followed. An active hectic set in and he rapidly wasted away. During the week before operation he failed so rapidly that it seemed he could live but a short time.

On May 26th, 1888, just two years ago, I operated. Making a free incision, I removed the spines and arches of the eighth, ninth, and tenth dorsal vertebræ. Outside the carious arch of the ninth was a half-ounce of thick pus, but within, and filling the vertebral canal, was a small quantity of inspissated pus and a large amount of dense neoplasm—evidently tubercular. It extended up and down the canal for two and a half inches, and was thoroughly curetted from the cord by Volkmann's spoon until sound bleeding tissue was left on every side. The cord was firmly compressed against the anterior wall of the canal. The wound was lightly packed with iodoform gauze and allowed to granulate. A plaster jacket was applied over all. On the eighth day sensation began to return in his thighs. In three weeks he began to move his legs and toes. His hectic left him. Pain disappeared at once and appetite returned. In six weeks he moved his legs well; in three months walked with crutches; in five walked without support, but with an ataxic gait; in eight had become robust and hearty and walked very well.

He resumed work and remained in perfect health for two years, walking as well as ever and without fatigue. Recently, after the confinement of the winter, he has had an abscess form in the cicatrix which had been so long healed, and I have had to curette a sinus remaining from it which led down to the bone. There has been no affection of the cord, however, and I hope very soon to

heal the sinuses, though they have a distinctly tubercular appearance.

CASE VI. *Pressure Paraplegia from Extradural Sarcoma; Operation after Eight Months; Resection of Arches of the Eighth, Ninth, and Tenth Dorsal Vertebræ; Complete Removal of Tumor; Death on Ninth Day.*—Professor D. A. MacG——, aged forty-two, of Toronto. The family history of this patient represents an unusually vigorous type and free of any inherited tendencies; he himself a man of excellent physique and with no acquired disease, and enjoying exceptional health except for one incident, repeated eight or nine times during as many years, namely, that he had vomited dark blood from his stomach, "not at any time more than a teacupful," and always after some particularly violent exercise, such as rowing, sawing, or shovelling snow, which caused kneading motion of his stomach. He never regarded the matter seriously, and only once sought advice. The last occasion was one year ago.

Three years ago, while placing a pedal under a heavy piano, the instrument was let down and pressed heavily on his back. He suffered pain several days. Six months later, while lifting the corner of a piano, he was caught by an excruciating pain in the back and felt as if he had stripped up a piece of muscle from the rib on his right side. No further pain or trouble ensued. About July 1st, 1889, he jarred his spine severely by attempting to spring upon a car platform, but, miscalculating the distance, alighted violently on his feet on the pavement. A week later he again jarred his spine by slipping on an icy place and plunging forward on his hands and knees. These are all the possible sources of trouble known to him. In July, 1889, he first felt a decided, though not severe, pain in his back at the site of the present trouble. His health began to decline. His bowels became difficult to move except by laxatives, which it was found necessary to make stronger and stronger. As the days passed on he found it difficult to urinate, and by great effort could only partially empty the bladder. A pain, mostly in the right side, centred in the back, and a "trickling sensation worked about the line where the girdle of paralysis now is." He kept himself tightly bandaged to relieve pain. He was believed to have muscular rheumatism, and, as he was losing strength rapidly, was urged to

take exercise. In two weeks he found it painful to rise and dress, but by effort did so, and, supporting his back with both hands, walked about the house. At the end of five weeks he found himself losing his ability to guide his limbs. His limbs grew weaker, and he walked pushing a chair before him for security. He had no temperature, but from sheer weakness he took no exercise for several days. After again being told he must exercise, he made one final effort on the evening of August 23d. He pushed the chair before him, but fell on his side. With indomitable will he made another effort and fell on his back. Then he crawled to a sofa and lay there all night. Immediately thereafter his paraplegia and insensibility were complete. One of his feet was blistered without his knowing it, and retention of urine required catheterization. During two months succeeding he recovered a little control of his bladder and improved somewhat in health.

In October, 1889, he was visited by Dr. E. C. Seguin in consultation. A diagnosis of pressure paraplegia was made and operation advised. A letter from Dr. Seguin, which I will cite later, gives the ground of differential diagnosis. About January 1st, 1890, he came to New York under the care of Dr. R. F. Weir. Drs. Seguin and Weir found on examination a slight fulness of the eighth dorsal spine, and advised a month or six weeks' orthopædic treatment, hoping that the pressure might be from Pott's disease, and that a natural relief of intravertebral pus might soon be expected and the paraplegia cured without operation. Dr. A. B. Judson adjusted a most comfortable spinal brace, and watched his course.

The looked-for improvement did not come. His cystitis grew worse. Absolute anæsthesia was established below the waist. The bowels, which had been constipated, became incontinent. He had a fortnight's illness with acute nephritis and a temperature of 102½° F. Dr. Weir being now in Europe, he was transferred to my care, as he had directed in case of decline.

March 13th. On examination his urine now had five per cent. of albumin. The anæsthesia line encircled the body, passed through the navel in front, the second lumbar spine behind, and a line at the sides three inches above the iliac crest. The muscles of the legs were in spastic contracture. The plantar, perineal, patellar, and other reflexes were exaggerated. The eighth, ninth, and

tenth dorsal vertebral spines were raised a little, and pressure on the right of them gave pain, as did pressure on the angles of the ribs on the same side. General condition fairly good for operation.

On March 20th, the day before it had been arranged to operate upon him, he had an unaccountable chill, with a temperature of 104° F. Prior to this it had ranged from normal to 100° F. His urine showed twenty per cent. of albumin and various casts.

On March 23d, another chill and profuse sweating. His stomach had rejected food, but soon improved. This attack lasted over two weeks before his temperature fell to normal. There were no pulmonary complications, but deep ulceration of the rectal wall was found, which he said had existed for two months.

This attack, as well as the one of six weeks before, was probably due to a slight septic infection from the rectal ulcer, which now healed under iodoform.

By April 16th he seemed again in fair condition to bear operation, though not nearly as well as before. Assisted by Dr. Weir and Dr. Farquhar Curtis, I incised from the seventh to the eleventh dorsal spines. The arches of the eighth and ninth, as well as the base of the spine of the eighth, were found somewhat crumbly and eroded by a softish dark growth which disintegrated the bone where it passed outward from the vertebral canal. The bone was unusually porous in the neighboring parts, not immediately involved in the tumor, and bled freely.

On removing the arches of the eighth, ninth, and tenth vertebræ, and the pedicle of the eighth, a firm, dark growth was found to fill the vertebral canal, compressing the cord to the left side and flattening it somewhat forward, so that it represented scarcely more than half its normal bulk. The tumor stopped abruptly at the ligamentum subflavum above the eighth, and extended downward an inch and a half. It was readily removed by blunt dissection from the dura, which was left with a quite natural appearance. The growth bulged backward between the arches, laterally between the pedicles, which it softened, and forward into the body of the eighth vertebra, and at the side into the subpleural space. From all of these sites it was removed by Volkmann's spoon. The pleura could be seen to rise and fall with respiration.

Not a trace of pus suggestive of tubercular caries was seen anywhere. The wound was packed loosely with

iodoform gauze. No plaster jacket was used. He endured the operation very well and was in excellent spirits for four days, when an occasional hiccough was observed. His wound was in perfect condition, and, the packing being removed on the second day, the sides were allowed to fall together.

On the fifth day the hiccoughing became rapidly worse, and he vomited everything except a little milk. On several occasions he vomited a little bloody fluid, such as he had done in former years. His bowels were freely acted upon, and his kidneys were doing quite well, with only five per cent. of albumin in the urine.

Every measure was resorted to to check the hiccough and vomiting. It seems that he was subject to attacks of this at times, and was relieved by soothing his stomach with slippery-elm water or gum arabic, both of which failed now. His vomiting continued also. By the eighth day the incessant hiccough and vomiting had exhausted him. His respiration became irregular, pulse intermittent. He became delirious, and died on the ninth day. His temperature for the last four days was nearly normal. The muscles of the legs began to react to electricity on the fifth day, the gracilis first, and many others on the sixth day. No return of sensation or voluntary motion, however, ensued. A careful examination of the tumor was made by Dr. J. S. Thacher, who found it to be a round-celled sarcoma, without trace of leucocytes, giant cells, or tubercular material.

Although this patient died exhausted largely by the hiccough, I suspect there was some morbid condition of the stomach, like ulcer. No post-mortem examination was permitted, except of the spinal cord exposed in the wound. This was found to have rounded up to nearly its full size during the nine days that had transpired since operation. The dura was quite natural, and on opening it the cord and membranes looked normal in color, form, and consistence.

Of these two cases, the first may be considered one of tubercular tumor, as that was the essential feature of it. In all probability it only involved the arches of the bone; after its growth in the canal it began to press on the cord. The neoplasm was dense cellular growth, not hypertrophied dura, for that was left when the former was curetted

away. The subsequent course of events has left no doubt of its tubercular origin. MacEwen reports two similar cases. My second case is of such special importance, as illustrating the differential diagnosis between myelitis of the cord and pressure paraplegia of tumor, that I take pleasure in incorporating a letter just received from Dr. Seguin relative to that question, and feel that further comments from me are useless:

"23 ABORN STREET, PROVIDENCE, R. I.,
May 13th, 1890.

"MY DEAR ABBE:—The diagnosis of compression myelitis was arrived at by deductive and exclusive reasoning. The dominant symptom, the 'signal symptom,' was the fixed side pain, which soon became a lumbo-iliac (?) pain. Then followed a gradually increasing paraplegia, without atrophy (at first), but with great increase of the reflexes. The anæsthesia was secondary during the first few months.

"Now, this symptom-group (at the end of October, 1889) was suggestive of a myelitis in the involvement of the lateral columns; a spastic paraplegia *plus* anæsthesia. The anæsthesia served to exclude primary sclerosis of the lateral columns. But the initial symptom, the fixed side pain, has, we well know, a very restricted meaning. It is never present in myelitis, or in sclerosis of any system of the cord. It surely indicates an extra-cordal lesion, involving in some way the posterior root or the mixed root of the nerve which supplies the seat of the fixed pain. I call this the dominant symptom, because it serves to locate the lesion (or the chief and primary lesion) outside the spinal cord at a certain level. This being granted, the other symptoms, which are apparently more striking, viz., slowly increasing paralysis and anæsthesia (the latter subordinate), and great increase of reflexes, would surely point to the existence of a lesion outside the cord, producing steadily increasing pressure on this organ at a limited point or level, leaving a healthy segment of gray

matter caudad of the lesion. The diagnosis turns upon the pressure of fixed pain in one side, and also, though with much less logical force, upon the comparatively slight anæsthesia, at a time when the volitional motor impulses were wholly arrested at a given level.

"A focus of central myelitis in the dorsal region would give rise to a paraplegia, with equal sensory or motor symptoms, but probably with greater sensory symptoms; and the side pain would be absent.

"Having established, easily I think, the diagnosis of compression of the cord by a growing lesion, the further diagnosis, viz., that of the nature of the compressing lesion, is extremely difficult, and often impossible. In a case which has exhibited external cancerous or sarcomatous tumors, the diagnosis of such a tumor of the dura or bones is not difficult. In cases where signs of pulmonary phthisis are present, or where there is a clear history of injury to the vertebral column, we are warranted in diagnosticating some form of 'Pott's disease' with caseous formations. In a syphilitic subject, the question of gumma of the dura mater should be considered. In cases without injury, or phthisis, or syphilis, or external tumors, we can only venture a diagnosis of primary tumor of the dura or bones by exclusion, but this is, after all, guesswork to a certain extent. Usually the tumor is not diagnosticated, as happened in the case of Commander J——, where I made a diagnosis of compression myelitis, *probably* from 'Pott's disease.' I do not include in this discussion those cases in which an evident large kyphos indicates the collapse of the bodies of several vertebræ from 'Pott's disease'; these are most easy of recognition.

"What I have said of the significance of the side pain will explain why I have always taught that great care should be taken to ascertain exactly whether the sensation in the patient's side (chest, abdomen, or groin) is a true pain, or a sense of constriction. The former has the specific value I have assigned it above, while the latter occurs in myelitis, meningo-myelitis, posterior spinal

sclerosis, etc. Either of these symptoms may be uni- or bilateral.

"Let me add it as my deliberate opinion that in cases of compression myelitis, except when due to syphilis and 'Pott's disease,' *improving* under appropriate treatment, an exploratory or curative operation is justified, and should not be too long postponed.

"Sincerely yours,

"E. C. SEGUIN."

Finally, two cases of section of certain of the sensory roots of the brachial plexus for neuralgia present what I supposed to be a unique operation. Soon after its publication, however, the case of Mr. W. H. Bennetts, of London, was published, narrating a similar operation for relief of sciatic neuralgic spasm, by division of the posterior roots of the first, third, fourth, and fifth lumbar, and first and second sacral nerves within the dura. The man was at once relieved of all pain, though the spasms continued and anæsthesia of an extensive distribution in the leg resulted. In a few days this area renewed its power of sensation, though the pain kept in abeyance. On the twelfth day the man died of apoplexy.

This operation was performed on December 24th, and mine on December 28th.

CASE VII. *Intractable Brachial Neuralgia; Nerve Stretching, Amputation, and finally Intradural Division of the Sixth, Seventh, and Eighth Cervical Nerves.*—This case was reported at the State Society Meeting (New York *Medical Journal*, February, 1889), and will be epitomized here.

For nearly two years before I operated, the patient, a man forty-four years of age, had suffered most intense neuralgia of the right brachial plexus, appearing in the forearm and hand. It began the night after a hard day's work putting a zinc lining in a large butcher's refrigerator. It grew worse and more paroxysmal. The hand became disabled and muscles somewhat atrophied. Dr. Dana and others agreed in the diagnosis of ascending neuritis.

Dr. Bull stretched the posterior interosseous and ulnar nerves in the spring following. The pain was no better.

The patient begged an amputation, and in July Dr. Bull removed the arm at the deltoid insertion in the humerus. The pain did not abate. The fingers seemed to draw and twist as if still on. He acquired the morphine habit, taking a half-grain hourly to subdue pain.

The possibility of the pain being caused by a tumor or inflammatory process near the origin of the nerve roots, or of dividing the sensory roots behind the ganglion, as proposed by Dr. Dana, who referred him to me, led to the following operation:

On December 31st, 1888, I removed the arches of the fourth, fifth, sixth, and seventh cervical vertebræ, and exposed more than two inches of the cord. No tumor or abnormality was felt. I then drew back the roots of the sixth and seventh nerves from the intervertebral foramina into the vertebral canal, made some electrical observations, detailed in the previous report, and then cut them across just outside the dura where sensory and motor roots join. The wound was lightly packed with gauze.

Recovering from anæsthesia, he still suffered pain, seemingly in the fingers. This region was supplied by the eighth cervical nerve. Forty-eight hours after the operation I placed the patient prone, under a good light and without anæsthetic, removed the gauze packing, and did the following operation: The dura and cord lay in the bottom of the wound, clean and dry. I punctured the dura and split it for an inch and a half, letting out two ounces of spinal fluid. This was painless and produced no appreciable sensation upon the patient or effect upon his pulse.

I now picked up the posterior roots of the eighth nerve within the dura, which was at the same level as the seventh outside, and cut a quarter of an inch from it. It gave him exactly the same pain when handled as he had experienced for two years or more, and I hoped we had at last gotten at the root of the trouble. I treated the seventh nerve likewise, then sutured the dura with fine catgut, and closed the entire wound. Immediate union was obtained, better even than by primary suture, and a fine linear cicatrix remains, as you see, upon the patient's neck.

The pain entirely changed in character, though he had considerable pain for the next ten days. It no longer went down into the fingers, but seemed to draw the stump. After eleven days he sat up and stopped his morphine.

Pain was even less after this. Yet it was paroxysmal and at times severe. The skin was anæsthetic, completely so, from the acromion process downward on the entire outer sides of the arm. There was partial anæsthesia of the anterior and posterior aspect of the arm and over the shoulder from the middle of the clavicle to the middle of the scapula, while the skin facing the axilla was rather hyperæsthetic. This condition has been maintained unchanged up to the present, a year and four months since operation. Another disturbance is of interest. Since the fourth day after the operation the vaso-motor disturbance is shown in the anæsthetic skin, which when pinched remains blanched for two or three minutes, and when pricked or scratched becomes suffused and forms wheals persisting ten or fifteen minutes. This condition still maintains. He now thinks he has as much pain as before the operation, and has gone back to taking a grain of morphine daily.

CASE VIII. *Intractable Brachial Neuralgia, Intradural Division of the Posterior Roots of the Sixth, Seventh, and Eighth Cervical and First Dorsal Nerves.*—Gabriel Z——, aged forty-five. Patient gives no history of rheumatism or syphilis, but has had malaria at one time.

In the latter part of 1886 he states that he exposed his arm at the window of a street car while he was in a perspiration, and he dates the beginning of his trouble from this time. A few days afterward he experienced sharp pains between his thumb and index finger. This grew steadily worse, and at length became located on the outer side of his forearm, and was accompanied by a feeling of drawing and twitching of the little and ring fingers as well. The pain extended up the forearm. It was intermittent, but of such severity as to cause him to cry out.

After two months he grew weak and nervous, lost his appetite, and suffered so much that he had to give up his business.

The attacks were paroxysmal, and the sensation described by the patient was that of a "drawing pain," the acute seizure leaving some continuous pain and soreness on the ulnar side of the hand and arm.

In August of the following year his ulnar nerve was stretched by Dr. L. A. Stimson. The pain grew steadily worse, the attacks coming on more frequently, at intervals

of half an hour. The pain spread over the forearm and hand, and in July, 1888, the ulnar nerve was exsected by Dr. Fluhrer. Following this the pain recurred in an exaggerated form, and the nerves of the brachial plexus were stretched in the axilla by Dr. Gerster. The paroxysms became even more severe, coming on every five minutes during the day and every half-hour in the night.

After the first operation the forefinger became drawn backward and the forearm wasted. The patient took to morphine in large quantity, with only temporary relief.

On February 9th, 1889, the patient came under my care. Examination showed him to be a rather intelligent man, of spare physique. He bears evidence of long suffering, and has a haggard expression. His attitude is peculiar; he sits with his head bent forward, and his body bent so that his left elbow rests upon his knee, his right hand grasping his forearm, the elbow semiflexed.

At intervals of a few minutes he is seized with violent paroxysms of pain which he describes as of a "drawing" character, extending from the fingers up the forearm, as though his fingers were being drawn away from his hand and his hand from his forearm. He has a habit of shouting with these attacks of pain, which has made him a source of dread in the neighborhood of his residence. Grasping his arm, he will walk the floor in agony for from three to five minutes until the pain subsides. The night attacks are similar to those of the daytime, although not so frequent. Sleep is, of course, only fragmentary. Urine showed specific gravity 1.009, no albumin.

The flexor actions of the hand are very feeble. The intrinsic muscles of the hand are atrophied, the thumb and middle finger being constantly in a sweating condition. He can raise his hand to his head. The deltoid is atrophied. Triceps in good condition. Infraspinatus atrophied.

He requires hypodermics of morphine about every two hours to render him at all comfortable—one-sixth of a grain.

On consultation it was thought possible to bring about sensory anæsthesia by operation upon the sensory roots of the brachial plexus.

On February 15th the patient was etherized, and I operated, assisted by Dr. Murray. The neck was shaved, exposing the occipital protuberance, and the patient was placed in a semi-prone position on his right side. Incision

was made to the left of the spinous processes from the third cervical to the second dorsal vertebra. The laminæ of the fifth, sixth, and seventh cervical and first dorsal vertebræ were cut away, exposing the dura. Palpation revealed no tumor or abnormal condition of the cord. The dura was punctured, and an ounce of arachnoid fluid escaped. The dura was then slit up for two and one-half inches. The cord appeared normal, except that it was slightly congested. The posterior root of the first dorsal nerve was then lifted with a strabismus hook and divided by scissors close to the cord. The free end was then caught by mouse-tooth forceps, and one-quarter inch was exsected. The sixth, seventh, and eighth cervical roots were similarly treated. The dura was then sutured with fine catgut. After suturing, the seventh and eighth cervical nerves were then stimulated by a weak faradic current outside the dura, Dr. Dana assisting. Stimulation to the seventh resulted in sharp muscular contraction, the scapula being jerked upward and the arm rotated inward. Current applied to the eighth caused the forearm to be sharply extended.

The wound was then sutured with deep and superficial sutures.

On the following day the patient passed a very comfortable time, using less morphine.

On the second day he had but one hypodermic and slept two hours, taking his nourishment well.

The third day he complained of pain in the shoulder and arm, but had a fairly comfortable day and slept for four consecutive hours.

On the fifth day he had severe pain in his head and some mental excitement; took nourishment well. This day there was recurrence of his sharp attacks.

On the seventh day wound was dressed and he was more comfortable. He was allowed to get up, as it was found impossible to keep him quiet in bed.

On the eighth day he suffered less from pain.

From the ninth to the fifteenth day the old pain continued, though less severe.

At the end of four weeks the patient was discharged in a condition of moderate improvement in the matter of pain.

The morphine habit had been entirely checked.

Two months after the operation I visited him at his

house. He was much improved in general condition and doing well. He still maintained the habit of leaning his head down, and seemed to be in pain. He was, however, easily diverted, and if entertained seemed free from paroxysms for considerable intervals. His actions suggested those of a man craving attention, and gave me the impression of one desirous of continuing the appearance of suffering. He had entirely stopped the morphine habit, and had stopped crying out as formerly.

Examination showed the arm and shoulder appearing as before. The muscular power was unchanged. Anæsthesia seemed complete on the left hand, front and back, up to an inch or two above the wrist, over the entire dorsum of forearm, and side and dorsum of upper arm halfway above the elbow to the shoulder. Above the middle of the arm to the shoulder there was no anæsthesia.

Dr. F. H. Strong, of Yonkers, under whose care this patient had long been, wrote me that he found him decidedly better, and that he thought the amount of physical suffering which he really felt was now comparatively insignificant.

Soon after this the patient had a severe attack of diarrhœa, and lapsed into a low, muttering delirium, showing no evidence of pain while in this condition. For one month he continued to have delusions. He then improved rapidly, and commenced having pains in the wrist, though not as before.

Two months subsequently he began again to suffer pain and howled during the paroxysms.

Three months after the operation his mind was perfectly sound, but he complained again of his pain and was despondent, but did not scream out.

The subsequent course of his trouble during the year has been a mild type of the same evidence of pain as before the operation. The pain does not now cause him to shout. The anæsthesia remains the same.

Dr. Thacher's pathological report of the resected roots says: "Inflammatory exudation quite marked at portions of the surface and less marked at a few points inside some of the roots. The changes are most marked in and around the root of the first dorsal."

The basis for this operation is the fact that sensory conduction is isolated in the posterior root, which is easily

operated on within the dura. Experiments recently made by Singer, Horsley, and others, to study the ascending degeneration after this section in monkeys, showed that a speedy and complete degeneration backward into the cord followed the section. This would give the desired destruction of an inflamed or diseased nerve to its very ultimate fibres. In my two cases there has remained all the anæsthesia obtained at the operation. Pain, however, has apparently recurred, though much milder. There is often simulated pain in those who have acquired the morphine habit, but in these two cases I believe it to be genuine. My conviction is that if all five roots had been cut the chance of recurrence would have been less.

It is of interest that in four of the eight cases about two ounces of spinal fluid were evacuated when the dura was cut, but that no disturbance followed. These all recovered. Horsley says the dura is highly sensitive, and that the patient should be deeply narcotized when it is cut. That this is an error is shown by the man who endured a two-and-a-half-inch cut in it without anæsthetic, and told me at the time that it was painless.

I cannot end this paper without emphasizing the advantage of the method of operation I have adopted. It is the most speedy, the least bloody, preserves *all the tissues* in and about the spines, which are replaced and give firmness to the back as well as preventing a gap that Nature must fill.

The incision should be on one side of the spines only, they being cut off and dragged to the other side, exposing the entire arch without dividing the interspinous ligament. The only bone-cutting instruments to be used are two narrow rongeurs, one curved, one straight, with a quarter-inch bite, and presenting a flat Gothic-arch aspect when the points are brought together. Any saw is dangerous, awkward, and inefficient. The rongeurs are by far the quickest.

While we are not warranted in taking a sanguine view

of the results of operation, yet surgery, with its possibilities ever looming up, ought not to occupy the ultra-conservative ground of the past in this field.

The scope of operative work may never be a large one, but it will probably not be as small as heretofore.

BACTERIOLOGICAL RESEARCHES IN YELLOW FEVER.*

By GEO. M. STERNBERG, M.D.,

SURGEON U. S. A.

(*Abstract.*)

In 1879 Dr. Sternberg, as a member of the Yellow Fever Commission sent to Havana by the National Board of Health, devoted himself especially to a search for the specific infectious agent in the blood of patients in various stages of yellow fever. The result of this research was negative.

Extended researches made during the past two years by the most approved bacteriological methods fully confirm this negative result. Exceptionally micro-organisms are found in cultures from the blood and tissues, even when the autopsy is made very soon after death; but the bacillus encountered most frequently has been identified as the *Bacterium coli commune* of Escherich, which is constantly present in the intestine of healthy persons, and consequently cannot be the specific pathogenic agent in yellow fever. Other micro-organisms associated with this are found so exceptionally and in such small numbers that no special significance can be attached to their presence.

The examination of thin sections of the liver, kidney, and other organs, stained by the most approved methods for demonstrating micro-organisms, gives results corresponding with those obtained by culture methods.

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* Read before The New York Academy of Medicine, June 5th, 1890.

That various micro-organisms are present in small numbers in the liver and kidney (and presumably in other organs) at the time of death is shown by preserving fragments of considerable size in an antiseptic wrapping, which destroys all micro-organisms which may have accidentally fallen upon the surface of the fragment, and prevents the entrance of germs from without.

Such a fragment preserved for forty-eight hours at a temperature of 27° C. (80.6° F.) always contains a large number of bacilli of different species which have evidently developed from scattered bacilli present in the organ from which it was taken at the autopsy.

These bacilli are, for the most part, anærobics, or facultative anærobics, and do not give rise to a putrefactive odor. The tissue containing them has a very acid reaction. Putrefactive organisms are also present, and pieces of tissue kept for a longer time give evidence of putrefactive decomposition.

The micro-organisms present in fragments of liver and kidney preserved in the way indicated have been carefully studied, and numerous comparative researches have been made, since my return from Havana, which show that those most constantly and abundantly present are not peculiar to yellow fever. In cases of accidental death and of death from other diseases, fragments of liver preserved in the same way have contained the same micro-organisms.

My bacillus N, a large anærobic bacillus, which for a time I thought might be the specific germ I have been in search of, I have found in these comparative researches, and am obliged to exclude it from further consideration from an etiological point of view.

One bacillus, however, which was found in a considerable number of cases of yellow fever, in pieces of liver preserved in an antiseptic wrapping, although not in very great numbers, has not been found in my comparative autopsies. This is a non-liquefying bacillus, which resembles the colon bacillus in its growth in flesh-peptone

gelatin, and in its morphology, although it is somewhat larger. It is also more pathogenic, especially for rabbits. This is my bacillus *x*. Not having excluded it by my comparative researches, I look upon it as being possibly the specific yellow-fever germ, but have not been able as yet to obtain any satisfactory experimental evidence that such is the case.

This same bacillus is found in the contents of the intestine, where it is associated with a variety of other bacilli, some of which are strict anærobics and some facultative anærobics. The most constant and abundant of these is the *Bacterium coli commune* of Escherich.

Comparatively few liquefying bacilli are found in the contents of the intestine or stomach, or in cultures from pieces of liver and kidney preserved for forty-eight hours in an antiseptic wrapping. The bacillus of Dr. Paul Gibier has been found in the contents of the intestine in a limited number of cases, but it has been absent in a majority of the cases in which I have made autopsies—forty-three in all—and when present has not been abundant as compared with the non-liquefying bacilli. Nor was it found in any considerable number of cases in my cultures from fæces collected during the lifetime of the patient.

There is, therefore, no good reason for supposing that this bacillus has anything to do with the etiology of yellow fever. And, as a result of my extended culture experiments, *I feel justified in asserting that yellow fever is not due to a liquefying ærobic micro-organism.*

The micrococcus which Dr. Domingos Freire presented to me as his yellow-fever germ at the time of my visit to Brazil, grows readily in flesh-peptone gelatin, and causes liquefaction of this medium. Its presence would therefore be readily ascertained by the culture methods which I have employed. It has not been present in a single instance in my cultures from the blood and tissues or from the contents of the intestine. It is

therefore excluded from consideration as being concerned in the etiology of yellow fever.

The *Tetragonus febris flavæ* of Dr. Carlos Finlay, of Havana, is a common atmospheric organism in that city, which I have obtained in cultures from the surface of the body of patients suffering from various diseases, and of healthy persons. I have not obtained it in my cultures from the blood and tissues, and consider it definitely excluded as the possible etiological agent in the disease under consideration.

The morphological characters and mode of growth in various culture media of the different micro-organisms referred to, and of others encountered in my bacteriological researches, will be demonstrated by projecting upon a screen my photomicrographs and photographs of colonies and test-tube cultures.

A detailed report, containing a full account of the researches made during the past two years in Havana, Cuba, and at Decatur, Alabama (1888), is now nearly completed, and will be submitted to the President within a short time.

EXPERIENCE WITH SKIN GRAFTING AFTER THE METHOD OF THIERSCH.*

By CHARLES McBURNEY, M.D.,

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When Reverdin, in 1870, first brought before the profession the method of skin grafting well known now all over the world by his name, an extremely valuable addition to our surgical resources was made. By its aid surgeons have been able, in innumerable cases, to procure sound healing of extended ulcerated surfaces which without it would have remained permanently open, and in a much larger number of cases have they very materially hastened the cicatrizing process. This method is one with which all are so familiar that, at the present time, the very briefest description of it is sufficient.

Given a large ulcerated surface, as that resulting from a burn, and one which has reached a stationary condition, attempts at further cicatrization having ceased, small bits of the superficial layers of healthy skin are removed with curved scissors from the patient himself, or from another individual, and are immediately transferred to the ulcerated surface. The bits of skin are usually about the size of a canary seed, although considerable variations in size may be safely made. These are carefully placed upon the previously disinfected granulations, always with the raw surface downward, and are gently pressed so that they will lie flat and firm. Many such grafts may be made at one time, the number being only limited by the size of the ulcer and the patience and strength of both

* Read before The New York Academy of Medicine, October 2d, 1890.

patient and surgeon. An appropriate dressing is then applied, and changed from day to day; and while some of the grafts will disappear, many will become adherent, will receive a supply of nutriment from the vascular granulations below, and will rapidly extend their areas, appearing to have the effect upon neighboring granulations of stimulating the formation of firm epithelium. When cicatrization ceases to advance, a second grafting may be done on other granulations, and this may be repeated again and again until the entire surface is covered with a horny surface. As already stated, this was a very valuable addition to surgical resources, and has accomplished an immense amount of good; but it is a slow and laborious process, not infrequently unsuccessful, and applicable only to granulating surfaces which have been previously brought to a properly healthy condition. The method of skin grafting suggested by Thiersch was brought before the profession only a few years after Reverdin's first publication, and has been constantly made use of ever since that time at Thiersch's clinic in Leipzig.* Many surgeons are familiar with it, and yet, if I may judge from the limited number of publications on the subject, and from the comparatively few reported cases of its use, the method has not yet been generally fully appreciated, nor has the knowledge of it spread as far as it deserves. I did not myself estimate the method at anything like its real value until I visited Thiersch's clinic and witnessed the wonderful results he was frequently attaining. Since that time I have neglected no opportunity to apply the process in suitable cases, and in doing so I have been most ably assisted at the Roosevelt Hospital by Dr. Hartley and the members of my house staff, all of whom are deeply interested in the subject. Beginning with the original method of Thiersch, we have endeavored so to modify it as to reduce the labor of the dressings and to increase their efficiency. It is with

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* G. R. Fowler: "Annals of Surgery," 1889, p. 179.

a view of presenting the results of our practical work that I am reading this paper to-night.

According to Thiersch, all strong antiseptics are to be avoided in skin grafting, as the effect of these upon the cellular elements, upon the capillaries, and upon the blood itself, is such as to seriously endanger, and even totally prevent, the immediate union between the grafts and the surfaces upon which they are placed. Thiersch himself uses absolutely no solution, excepting one of common salt in water, in the proportion of six parts to one thousand, or about one and a half drachms to the quart. I am convinced that it is important that this solution should be sterilized. The water used should be distilled and the solution should be boiled just before using. My own practice has been to use a solution of bichloride of mercury freely in disinfecting the ulcerated surface, and also the surface from which the grafts are to be taken, afterward completely washing away all of this solution with the saline one of Thiersch just before operating. I have also used soap and water thoroughly in beginning the cleansing process. All hair should be removed from the areas involved, by careful shaving. In some cases grafts may be placed with success directly upon the unbroken surface of healthy granulations, but a much more certain method of securing success, and, according to Thiersch, one essential to the avoidance of subsequent contraction, is to remove, by curetting, the superficial layer of granulations and also the tough cicatricial edge of the ulcer. A bleeding surface is thus produced on which many open capillaries exist, and which is very favorable to the immediate union of the grafts. To bring about the same result I have found it more rapid and convenient to use the knife. With a sharp scalpel an incision nearly or quite through the true skin is rapidly made entirely around the ulcer, just outside of its thickened border. With the same instrument the whole area included by the incision, that is, the granulating surface and thick edge, is shaved off with great ease and rapidity. A smooth, bleeding, healthy

surface of a considerable depth is thus produced. This surface is immediately irrigated with the sterilized salt solution, and compresses of sterilized gauze are pressed upon it to control bleeding. The question of hemorrhage at this stage in the operation is one of considerable importance. If grafts are placed while bleeding still goes on, the risk of failure is much increased. Accumulations of blood, even small ones, beneath the grafts frequently induce their necrosis, and saturation of the dressings with blood is favorable to infection. But all ulcerations, and particularly those on the lower extremities, when shaved, are liable to bleed actively for a long time; so long a time, indeed, as to sometimes seriously endanger the patient's condition, or at least to wear out the surgeon's patience. E. Fischer made the observation that grafts united more readily when Esmarch's constricting band was applied above. I feared at first that the complete anæmia thus induced might endanger the vitality of the grafts. I have found, however, that the use of Esmarch's band entirely disposes of the question of hæmorrhage, shortens the whole operation very greatly, and does not in the least affect the vitality of the grafts. When, therefore, the situation of the ulcer is such that the band can be applied, it is put on above the whole field of operation, thus rendering the process a nearly bloodless one. The grafts may then be immediately applied and the dressings put on. After operating upon the extremities they should be placed in an elevated position to favor hæmostasis. In several instances I have allowed the Esmarch band to remain on the limb for an hour or more after the operation was finished, and always with a favorable result. If vessels of sufficient size to require special treatment are opened, they should be closed by torsion rather than by ligature. The knot of even a fine catgut ligature prevents close adaptation of the graft at that point.

The grafts themselves illustrate most remarkably the originality of Thiersch's method. They consist of thin

slices removed by shaving parallel with the surface. They may of course be taken either from the patient or from another individual. I have myself always used the patient's own material. Any broad knife may be used in cutting the grafts, but no instrument is so good as a broad razor with a very delicate edge. With this instrument, shavings no thicker than a sheet of note paper, four or five inches long and an inch broad, may with a little practice be readily made. The surface from which grafts are to be taken should have been thoroughly cleansed and shaved, and then be wet with the salt solution. The most favorable and convenient points are the front and outer parts of the thigh, and the outer surface of the upper arm. If the surface to be grafted is upon the head or neck, the upper arm of the same side should be selected for the grafts. If the leg is to be grafted, the thigh on the same side is most convenient. If the breast or abdomen is to be grafted, the thigh also furnishes the grafts most conveniently and in the largest quantity.

The operator should stand facing the patient. With his left hand grasping the limb from below, he stretches the skin snugly about the limb. His assistant, using both hands, stretches the skin from above downward. It is impossible to take good grafts from a lax surface. Taking the razor in the right hand, the operator wets it in the salt solution, and, applying the edge at the most distant part of the stretched skin, with a rapid shaving motion toward himself shaves off as thin and long and wide a shaving as he can. Four or five inches in length and an inch wide are about the dimensions of a first-rate graft. In regard to thickness, considerable variety may be safely indulged in. Some grafts may be as thin as tissue paper, and others as thick at some parts as cardboard. All will do well, but no graft will do well that is so thick as to have fat upon its under surface. While cutting, the graft folds up upon itself on the surface of the razor, and when a sufficient length has been reached the edge of the razor may be turned outward and the graft so severed from its

connection with the skin, or, generally more conveniently, an assistant will sever it with scissors. If the graft is once dropped from the razor great difficulty is met with in handling it. If this accident happens, it takes less time to cut a new graft than to rearrange the old one. The graft being then piled up on the razor, a few drops of salt solution should be dropped on it and the instrument be immediately carried to the edge of the surface to be grafted. With a probe, one end of the graft may be slid off the razor to the raw surface, and, being held there, the razor may be slid from under it across the surface, so as to unfold the graft and deposit it in place. If not exactly in place, it may readily be slid about with two probes until it lies accurately in position. The graft should be so placed that its end will follow up the perpendicular edge of the prepared surface, and reach very slightly on to the uncut skin. In this manner graft after graft is placed, great care being taken that their edges are in neat apposition to one another and are at no point folded under. From time to time a little salt solution should be sprinkled on the grafts already placed, that they may not become too dry. The entire surface to be grafted should be covered. Moisture is absolutely essential to the continued vitality of the transplanted skin; therefore a dry dressing is inadmissible. Strips of Lister protective or of thin rubber tissue, about an inch wide and long enough to a little more than cross the grafted surface, should then be laid like shingles, overlapping one another, over the entire area. These strips should have been sterilized in the bichloride or carbolic acid solution and then drawn through the salt solution. They should be gently pressed flat upon the grafts.* Any soft sterilized material may be used as a compress over the protective, but nothing is better than loose handkerchiefs of sterilized gauze. A mass of this material, wet with the salt solution, should be piled

* Strips of rubber tissue are used rather than a single large piece, in order that the salt solution with which the gauze is wet may more readily reach the grafts.

up over the protective or rubber tissue to prevent rapid evaporation, and finally a well-applied gauze bandage.

The surface from which grafts have been taken now requires attention. Up to a recent period I have habitually treated this surface by dusting it with iodoform and covering that with iodoform and bichloride gauze. With one exception, which will be referred to later, this surface has always healed well, but it has often been quite painful, and has frequently required a longer time to heal than the grafted surface itself. One of my assistants, Dr. Schultze, recently suggested that this surface be wet with salt solution, and then be completely covered simply with rubber tissue, with sterilized gauze above. This method has given great satisfaction, healing going on very rapidly and entirely painlessly. In a recent case, a very large surface on the thigh from which grafts had been taken was treated in this manner and was entirely healed on the eighth day.

I have made no mention as yet of the use of anæsthetics while performing this operation, and I notice that the writer of a recent article on this subject expresses a fear that they may act unfavorably on the grafts. I have used ether in every case operated upon, and, so far as I can see, without the slightest unfavorable effect.

The variety of lesions to which Thiersch's method is adapted is very large. It includes all ulcerated surfaces upon any part of the external surface, and the process is destined, I am very sure, to displace all of the older methods, many of which are in constant use to-day. Varicose ulcers, ulcers produced by burns, by extensive wounds, by sloughing processes, can be all rapidly and successfully healed by this method. But those who are not familiar with the method would be surprised to see how perfectly and successfully these grafts can be immediately applied to the denuded surface produced by extensive operations for the removal of malignant and other tumors—to any raw surface, in fact, which cannot be covered by the adjacent skin. How often it happens that the complete removal of

a cancerous breast, particularly in secondary operations, or the excision of a broad tumor from the head or face, necessarily leaves a very large exposed surface to heal by the slow process of granulation and cicatrization. In not a few cases malignant disease reappears before the wound produced by operation has healed, to the disgust of the surgeon and the despair of the patient. Upon such surfaces grafts may be placed as soon as the primary operation is finished and hæmorrhage has ceased. And it is astonishing to see the variety of tissue to which the grafts will firmly adhere. Muscle, fascia, cartilage, and even bare bone may all be successfully grafted. The most favorable surface is a clean muscular one, and the least favorable the surface of compact bone. In one case, after the removal of an extensive carcinoma of the face, a large area of lower jaw was completely bared. Grafts laid over this surface adhered nearly perfectly throughout, and at the end of a week those that covered the bone were found to be soundly attached to it. In another case, a surface several inches in diameter on the back of the forearm included a number of exposed extensor tendons. The grafts applied in this case were more than usually successful, and complete healing was rapidly attained. The breast cases have especially interested me, and I have come to feel entirely at liberty to make extremely wide and thorough removal of the disease in this situation, knowing that I could at the same time cover the surface without resorting to any plastic operation by sliding flaps, and without running the risk of having to treat a large ulcer for many months after operation. The method adopted in the breast cases has been as follows: First, a complete removal of disease, including as much surrounding skin as seemed desirable to avoid recurrence. Then a closure by suture of such parts of the wound as could be sutured without too great tension.

There would still, perhaps, be left an exposed area of chest wall of from four to six inches diameter. The depth of this wound I have diminished by a considerable

number of catgut sutures so applied as to partially invert the skin edge, and bring these down nearly to the muscular level. Upon this diminished and shallow wound the grafts have been laid so as to entirely cover it. The wound has then been dressed in the manner already described, and the arm, bent at a right angle at the elbow, bound to the thorax. After the primary dressing, the management of the cases has been varied gradually from the original method. I learned from Thiersch that the gauze placed immediately over the protective should be kept constantly moist with salt solution, and that to effect this it was necessary to moisten the gauze every four hours. This was a very laborious process, and I have gradually extended the time between the applications of salt solution, until now it is done only once in two days. This seems sufficiently often, and the cases treated in this manner have done perfectly well. At the end of forty-eight hours, then, the dressings are all removed, excepting the protective which lies next to the grafts. A fresh wet dressing is applied and changed again after two days. By this time there is usually some formation of pus, or at least of a purulent-looking exudation; in fact, I have seen no case go on for a whole week without any pus. If it exists in considerable quantity it is removed by absorbent gauze, and the protective removed and fresh material applied. The protective may be lifted and the grafts examined without danger at any time after the second or third day. If successful they look smooth and pink, and if unsuccessful at any part they look shrivelled and yellow or brown, and may die at some points. But even when only partially successful, the granulating spots have quite rapidly healed over under ordinary treatment. A dry dressing applied too soon to the grafts destroys their vitality through the absorption of moisture, and we have made it a rule to leave the protective in place for from ten to fourteen days. At the end of this time the protective has been removed and an ordinary antiseptic dressing applied; granulations, when

exuberant, being touched with nitrate of silver or dressed with balsam of Peru.

CASE I. *Large Indolent Ulcers originating in an Extensive Suppurative Prepatellar Bursitis.*—M. B——, a male, forty years of age, entered the Roosevelt Hospital on December 30th, 1889. Persistent treatment totally failed to diminish the ulcerated areas.

February 15th, 1890. Esmarch's constricting band applied just below the left groin. The surfaces of the ulcers, three in number, were shaved, and the edges completely excised. Grafts taken from the left thigh were applied, then the dressing already described, the limb was elevated, and the patient sent to bed with the constricting band still on. The band was removed a few minutes later.

March 4th. Grafted areas all soundly healed, except ing that a few very small granulating spots remain.

March 16th. Healing complete.

CASE II. *Extensive Recurrence of Carcinoma in and about the Cicatrix left by a former Operation on the Left Breast.*—Mrs. S——, aged forty. August 20th, 1890. A very free excision of all diseased tissue was made. A few sutures were applied at the inner and outer angles of the wound, but an open area about five inches in diameter remained uncovered. To this surface grafts taken from the left thigh were applied on September 16th. The entire surface was soundly healed.

CASE III. *A large Epithelioma of the Malar and Anterior Temporal Regions of the Right Side of the Face.*—Miss M——, forty-one years of age, came under my care in April, 1889. The notes of this case have been lost, but after excision of the tumor the exposed surface was large and deep. Cicatrization was very slow, and in May, 1889, grafts were applied to the entire surface. The operation was very successful. I saw the patient this morning, and the grafted surface is perfectly smooth and sound.

CASE IV. *Large Cysto-sarcoma of Right Breast.*—Mary B——, aged fifty-two, entered the Roosevelt Hospital on June 29th, 1890.

July 6th. The tumor was removed by free excision, no available skin remaining to cover the exposed surface left, which measured four by five inches. A few sutures

at the angles of the wound were applied. Grafts taken from the right thigh were immediately applied. In this case only about half the grafts were successful. The whole wound was soundly healed on August 26th.

CASE V. *Lupus of the Cheek measuring three by two and one-half inches.*—Ellen H——, aged thirty-eight, entered the Roosevelt Hospital on August 4th, 1890.

August 8th. Complete excision of the disease. Grafts were immediately applied from the right thigh.

September 1st. Complete healing had occurred. In this case the new surface is decidedly keloidal in character.

CASE VI. *Large Varicose Ulcer over Inner Malleolus of the Left Ankle.*—Eliza E——, aged forty-six, entered the Roosevelt Hospital on June 3d, 1890. Margin unhealthy and base sloughy.

June 13th. The ulcer was completely excised. Grafts from the thigh immediately applied.

July 22d. Complete healing had occurred.

CASE VII. *Recurrent Sarcoma of the Right Breast.*—E. S. M——, aged forty-three.

May 29th, 1890. Thorough extirpation of diseased tissue, leaving an open surface measuring about five by four inches. Immediate grafting from the right thigh.

On June 29th a few minute granulations remained. The rest of the surface had healed.

CASE VIII. *Large Indolent Ulcer on Left Leg.*—F. L——, forty-six years of age, entered the Roosevelt Hospital on July 17th, 1890. The ulcer was about ten inches long and two inches wide. It had existed for eight years.

July 18th. The ulcer was excised, and grafts from the left thigh were immediately applied.

August 6th. Completely healed. (Patient exhibited.)

CASE IX. *Large, Hard, Ulcerated Carcinoma of Right Breast.*—Mary C——, forty-five years of age, entered the Roosevelt Hospital on June 19th, 1890.

June 27th. Extensive removal, leaving a surface which measured five inches in diameter, and which could not be covered by any ordinary means. Immediate grafting from the right thigh. About half of these grafts failed, doubtless owing to the fact that the first dressing was left unchanged, as an experiment, for five days.

August 12th. The entire grafted area was soundly healed.

CASE X. *Highly Developed Condition of Ectopion Vesicæ.*—W. W——, five years of age.

November 19th, 1889. Wood's flap operation was done, and, to diminish the raw surface left, sutures were applied wherever they could be useful. A large uncovered wound still remained, which was immediately grafted from the thigh.

December 10th. The entire surface was healed.

CASE XI. *A large Ulcer of the Leg originating in an Extensive Injury.*—J. D——, aged nineteen, entered the Roosevelt Hospital on October 19th, 1889. The ulcer had existed for six years, and had received much treatment.

December 28th. The surface of the ulcer was shaved and grafts applied.

January 20th. Three small points of granulation only remained open.

February 12th. The whole surface was firmly closed.

CASE XII. *Flat Ulcerated Epithelioma of Left Forearm.*—Mary G——, aged sixty, entered the Roosevelt Hospital on July 5th, 1890.

July 11th. Complete excision, leaving a surface which measured four by five inches. Grafts were immediately applied from the left thigh.

August 5th. Complete healing had occurred.

CASE XIII. *Large Sarcoma of Left Breast.*—H. N——, aged fifty, entered the Roosevelt Hospital on June 4th, 1890.

June 6th. Complete and free excision, leaving an exposed surface measuring about five inches in diameter. Grafts from the left thigh were applied.

July 21st. The entire surface had healed.

CASE XIV. *Very large Carcinoma of the Left Breast.*—C. F——, aged forty-four, entered the Roosevelt Hospital on July 10th, 1890.

July 11th. Complete removal of the tumor. The uncovered surface which remained measured five by four inches. Grafts from the left thigh were applied.

August 5th. The wound had completely healed.

CASE XV. *Extensive Laceration of Right Forearm, re-*

sulting in an unhealthy Ulcer measuring three by five inches. —A. W——, aged fifty, entered the Roosevelt Hospital on March 21st, 1890.

April 23d. The ulcer was shaved and grafts from the thigh were applied.

May 4th. Only a few minute points of granulation remained.

CASE XVI. *Carcinomatous Ulcer of the Left Cheek, about one and one-half inches in diameter.*—J. M——, fifty-six years of age, entered the Roosevelt Hospital on January 24th, 1890.

January 25th. Complete excision and grafting.

February 17th. Completely healed, excepting one minute granulating spot.

CASE XVII. *Extensive Burns of Penis, Scrotum, Left Thigh, and Back.*—W. O——, fourteen years of age, entered the Roosevelt Hospital on July 2d, 1890. Immediate treatment, carbolized oil, etc.

August 8th. Three unhealed ulcers remained, the largest being on the antero-internal surface of the left thigh. This ulcer measured eight by six inches. Grafts taken from the right thigh were applied.

September 1st. Two small areas of granulation, the size of a quarter of a dollar, were the only unhealed points. These were dressed with balsam of Peru.

CASE XVIII. *Keloid over the Right Patella, aud Indolent Ulcer following Suppurative Bursitis.*—M. D——, sixteen years of age, entered the Roosevelt Hospital August 4th, 1890.

August 6th. An area measuring two and one-half by one-half inch was excised, grafts from right thigh immediately applied. Complete healing occurred on September 12th.

CASE XIX. *Ulcer on the Inner Aspect of Right Leg.*—B. C——, nineteen years of age, entered the Roosevelt Hospital on August 14th, 1890. The ulcer had existed for one year, and measured one and one-half by one and one-quarter inches.

August 15th. Grafts from the right thigh were applied.

September 15th. Entirely healed.

CASE XX. *Large, unhealthy Ulcers of Right and Left Legs measuring four by five inches.*—Ira F——, aged twen-

ty-nine, entered the Roosevelt Hospital on December 15th, 1889. One of these ulcers had existed eighteen years. Preliminary antisyphilitic treatment.

January 23d. No improvement having occurred, the ulcers were shaved, the edges excised, and grafts from the thigh immediately applied.

February 15th. The entire ulcerated surfaces were healed, with the exception of one spot of granulation the size of a three-cent piece.

This patient developed erysipelas in the surface from which the grafts had been taken, and was transferred to the pavilion at Bellevue. He recovered from his erysipelas and left Bellevue subsequently, entirely well.

CASE XXI. *A prominent Circular Epithelioma, of the Size of a Silver Half-dollar, situated on the Right Lower Eyelid.*—E. W——, aged thirty-nine, entered Roosevelt Hospital on June 2d, 1890.

June 6th. The tumor was excised without cutting the cartilage of the lid. A single graft from the arm was applied to the exposed surface.

June 13th. Complete and sound healing had occurred. The surface was smooth, and only differed from the surrounding integument in being slightly darker in color.

CASE XXII. *Epithelioma, two inches in diameter, over Right Temporal Region.*—A. S——, fifty-nine years of age, entered the Roosevelt Hospital on December 10th, 1889.

December 21st. Complete excision, leaving a surface measuring three by four inches. Grafts from right thigh immediately applied.

January 12th. Completely healed, excepting a few granulating spots.

CASE XXIII. *Ulcerated Carcinoma of Right Breast.*—R. R——, forty-four years of age, entered the Roosevelt Hospital on April 25th, 1890.

April 26th. Complete removal, leaving an uncovered surface measuring about four inches in diameter. Grafts from left thigh applied. About half the grafts failed to unite. This patient was lost sight of May 14th, with the surface largely healed.

CASE XXIV. *Large Carcinomatous Ulcer of the Left Side of the Neck and Face.*—J. F——, forty-three years of age.

December 28th, 1889. An extensive dissection was re-

quired to remove the disease, and a deep cavity measuring three by four inches was left after the excision. The descending ramus of the lower jaw was extensively bared, and also the mastoid process and the large vessels of the neck. Grafts to cover the whole surface were taken from the left upper arm ; all of these grafts adhered perfectly, even those applied to the exposed bone, with the exception of an area about one inch in diameter next to the ear.

January 21st. Almost the entire surface was soundly healed. The patient was then lost sight of.

I have reported here twenty-four cases, representing a considerable variety both as regards the size and the character of the surface grafted. Most of the patients were ordinary ward patients received into the Roosevelt Hospital, and were all operated upon either by Dr. Hartley or myself.

The length of time occupied in attaining sound healing has varied from one week to two months. Some of the best results have been accomplished on the most extensive surfaces. In regard to the permanency of the grafts, I may say that it is remarkably good. Most of the cases reported have been kept under observation, and in no case has anything more than a very minute breaking down of the new tissue been noted. Of course, no claim is made that Thiersch's grafting will prevent the return of malignant disease, but it is certainly much to be desired that after all operations on malignant disease the wounds should be healed at the earliest possible moment. The application of the method will, I feel sure, become constantly more extensive, and no one who gives it a trial can fail to be convinced of its very great value.

I should like now to present a few cases for your inspection. My house surgeon, Dr. Tousey, has kindly brought them here.

HYDROPHOBIA—ITS CLINICAL ASPECT.*

By LANDON CARTER GRAY, M.D.,
PROFESSOR OF NERVOUS AND MENTAL DISEASE OF THE NEW YORK POLYCLINIC.

When it became my official duty, as Chairman of this Neurological Section, to provide some subject of general interest for one of the general meetings of the Academy, I could think of no available one more opportune than hydrophobia, as the startling claims of Pasteur, and the multiplication of his so-called institutes throughout the world, have awakened fresh interest in a disease that has excited the curiosity and the fear of mankind from time immemorial. Moreover, the sensationalism of the newspapers about every fresh bite of an animal, and the uncertainty of physicians who have seen only one case or none at all, would be additional reasons for a discussion of this kind.

At the very threshold of the subject we are confronted by the question as to whether there is such a thing as rabies,† or hydrophobia. There has been much and bit-

* Read before The New York Academy of Medicine, October 16th, 1890, in a discussion upon Hydrophobia.

† There should be considered briefly some details of nomenclature about which much confusion prevails. By usage the term rabies is given to the disease in the animal, while the malady in man is termed hydrophobia. This was so stated in Fleming's book in 1872, and has since been adopted by the standard dictionaries and writers. I see no reason, however, why it should not be called rabies in the human subject. The disease was termed lyssa by the Greeks. meaning a worm, a term given to the frænum of the dog's tongue, which was supposed in the early days to be a worm, and was extirpated as such. This superstition lingered even as long as the early part of this century, when a Russian physician, by the name of Marochetti, claimed that in all cases of hydrophobia pustules appear under the tongue at a certain time of the incubation of the disease. The German writers still retain this name of lyssa. The spread of an infectious disease among animals is called an epizoöty (from ἐπί, upon, and ζῶον, an animal); just as the spread of an infectious disease in the human being is called an epidemic (from ἐπί, upon, and δῆμος, the people.

ter discussion upon this point during the last few years. Those who would answer this question in the affirmative allege as proof the fixed belief of ages, and the many epizoöties of which we have historical record. The very earliest writers speak of rabid dogs, and they were known as far back as the days of the Asclepiadæ, the descendants of the God of Medicine, Æsculapius; while there are very few parts of the world, whether in the tropics or the arctics, in which an outbreak of it has not been described at some time or other. At times it has raged with great fury, as in Northern Germany from 1851 to 1856, when at Hamburg alone six hundred cases were observed among dogs, two hundred and sixty-seven occurring in the year 1851. In England, in 1856, it raged severely among dogs and in a herd of deer, destroying nearly one hundred of the latter. In Würtemberg, according to the veterinary professor Hering, from January 1st, 1840, until the end of February, 1842, it affected two hundred and thirty dogs and twenty-one bitches. In 1866 there were thirty-six deaths from hydrophobia reported by the Registrar-General of Great Britain. It has never been very prevalent in this country, although local epizoöties of it have been reported from time to time. It is stated that it is now raging in a herd of deer belonging to the Marquis of Ipswich.

In almost all of these epizoöties human beings have been bitten by the rabid animal and have died, although it has never been spread from one human being to another, as is the case in animals. It would seem, therefore, to be conclusively proven that there is a disease which can rapidly exterminate a kennel of dogs, a flock of sheep, a drove of hogs, or a herd of deer, and which can render the timid skunk, badger, and fox ferocious and dangerous, and make of the wolf a beast of prey as fierce as a royal Bengal tiger. It would also appear to be conclusively proven that many people have died after being bitten by these rabid animals. But there has sprung up in this country, in the last few years, a small number of very

decided opponents of these claims, such as Spitzka, Brill, Dulles, Stockwell, and others; and, unless I am very much mistaken, they have voiced to some extent the feeling of scepticism that prevails among American physicians. In the first place, they say, many mistakes are made in diagnosis, so that animals are said to have rabies when in reality they have some one of many disorders that may cause similar symptoms, such as epilepsy, angina or sore throat, gastritis and enteritis, distemper, foreign bodies in the mouth and throat, tetanus, worms in the frontal sinuses or in the small intestines, and ulceration of the internal ear. In certain countries, extending from the sources of the Vistula to the Carpathian Mountains, there is endemic a disease known as the "Plica Polonica," attacking mankind, horses, cows, sheep, wolves, foxes, and dogs, in the latter closely resembling rabies, although the bite of these animals is not followed by hydrophobia. Spitzka has introduced into the skull-cavities of dogs such non-rabic material as the spinal cord of a healthy calf, common yellow soap, and stale horses' urine, producing a purulent meningitis and meningo-encephalitis, and the symptoms of dumb rabies. Miller has reported three cases of seeming rabies in the cow, in which clusters of the parasite known as *Strongylus tetracanthus* were found in the intestines. It is not unreasonable to suppose that the bite of an excited animal may be very dangerous, as it is an elementary fact in physiology that the secretions of the human female breast are at times fatal to the child. My friend Dr. E. F. Brush informs me that one of the earlier Borgias obtained a subtle and most dangerous poison by dipping a ring in the saliva of a hog which had been hung up and kept excited to a blind fury for a long time. The variations are almost incredible that are alleged to exist in the period of incubation of hydrophobia. Fleming states that the average in England has been from thirty to fifty-nine days. According to Tardieu, it may vary from one month to twelve. Trousseau says that it may run from a few days to a year. In Algiers it varies

from twenty to two hundred days. Sauvage speaks of a peasant who was suffering from the most pronounced hydrophobic symptoms on the third day after being bitten by a mad wolf. Chirac tells of a merchant who was bitten by a mad dog at Montpellier, afterward resided ten years in Holland, and was suddenly attacked by hydrophobia on his return to France and learning of the death of a brother cadet who had been bitten by the same animal. Finco, of Padua, narrates the case of a young woman who became hydrophobic fourteen years after receiving a wound from a rabid dog. Even Gowers states that the period of incubation is longer and more variable than that of any other known acute specific disease, and believes that many well-authenticated cases have occurred eighteen months after the wound. This extreme variableness in the period of incubation lends credence to the belief, firmly held by many competent observers, that death from fear can occur with symptoms closely resembling the true disease. These are the cases of so-called lyssaphobia or hydrophobia.

Although a recent writer has characterized such a belief as childish, it is yet a matter of clinical observation that great psychical shock can produce mental disease and death.

I have a patient in whom an attack of melancholia of eighteen months' duration was caused by her lover being suddenly thrown from his horse and killed before her eyes. I know of another case in which a woman's husband, who had been supposed lost at sea, suddenly appeared before her, when she became speechless, mumbling, and, preserving only consciousness enough to gently caress his face for a few hours, sank into a coma and died. A patient of mine with transverse myelitis, who had improved greatly, was much frightened by his room catching fire, and in a few hours he became delirious, and within twenty-four hours he was dead.

I have under my care at the present moment a stalwart, resolute young man, to whom the Secretary of the Trea-

sury has just presented one of those rare medals of gold that are only given to those who save a life at imminent danger to their own; and this man, notwithstanding he had nerve enough to plunge into the raging surf to rescue a fellow-being, has not been able to do a day's work since, suffering from those well-known symptoms which Oppenheim describes as traumatic neurosis, and which occur so frequently after railroad accidents. If these instances be not sufficient, a perusal of Tuke's book on the influence of mind on the body ought certainly to convince any impartial mind that such an overwhelming fear as is that of hydrophobia is quite sufficient to induce death, the more especially as a number of suicides have been committed from this very dread. Very few men in this city of New York, or, indeed, in this country, have seen a case of hydrophobia. Indeed, one of the great difficulties that I have encountered in arranging this debate has been that there is not a neurologist in New York City who has seen more than one case in his own practice, although some few have seen two or three cases at the great hospitals, while many men of great experience have seen none at all.

For my part, in an experience of twenty years as student and practitioner, I have never seen but one case, and, to be guilty of a Hibernianism, I would not swear to that. In this city of New York, from 1855 up to the present time, the number of deaths from hydrophobia have been as follows, according to the figures very kindly furnished to me by Dr. John T. Nagle, viz.: 1855, 4; 1856, 3; 1857, 2; 1858, 0; 1859, 2; 1860, 0; 1861, 5; 1862, 1; 1863, 3; 1864, 1; 1865, 3; 1866, 2; 1867, 4; 1868, 1; 1869, 5; 1870, 3; 1871, 7; 1872, 6; 1873, 0; 1874, 5; 1875, 0; 1876, 5; 1877, 4; 1878, 2; 1879, 0; 1880, 0; 1881, 3; 1882, 1; 1883, 1; 1884, 1; 1885, 0; 1886, 0; 1887, 1; 1888, 0; 1889, 1.

Thus, in a period of time covered by thirty-five years, there have been seventy-six deaths; in nine of these years there have been no deaths, and it has twice happened that

for two years at a time there was not a death. One of the deaths in 1874 is that of a man who was bitten by a dog supposed to be rabid, and a coroner's inquest was held, the cause of death being stated to have been lyssaphobia. I come in contact with many physicians from different parts of this country, and it is very exceptional to have one tell me that he has ever seen a case of hydrophobia.

From this necessarily cursory review of the *pro* and *con* of the subject, we may draw the conclusion, I think, that frequent mistakes are made in the diagnosis of rabies and hydrophobia; that death can be caused by the fear of the disease; that the symptoms of the so-called dumb rabies may be caused by simple purulent meningitis and meningo-encephalitis; and that very few cases of either rabies or hydrophobia have been observed in New York City or in the country at large. Admitting all this, however, the question still remains as to whether there is a true rabies or a true hydrophobia. For my part, I believe that there is a disease, running a fatal epizoötic course, in the dog and other lower animals, and capable of being communicated to the human being and causing death, although the evidence of this will rest mainly upon the pathological and experimental considerations which will be presented to you by Dr. Dana, Dr. Biggs, and others. Whether you call this rabies or something else is a matter of entirely secondary consideration.

If we may trust the present writers upon the subject, the symptoms of furious rabies in the dog are as follows: Alteration in the animal's demeanor, so that it becomes sullen, shy, and restless; hallucinations of sight, so that imaginary objects will be bitten or snapped at; strange and erratic movements, indicative of mental aberration; transition from obedience and affection to semi-delirious outbursts, or to frenzy and fury; irresistible tendency to roam, so that great distances may be covered in a surprisingly short time; great tendency to bite; greatly increased salivary secretion, which becomes thicker and

more viscid; a peculiar howl, which is described by Fleming as being neither a bark, howl, growl, nor snarl, but a curious jumble of all—the howl being predominant; finally exhaustion, paralysis, and death. In dumb rabies there is the characteristic paralysis of the lower jaw, so that the dog is incapable of either eating or drinking, or of closing the mouth; sometimes also a certain amount of tumefaction of the pharynx and tongue, and finally paralysis of the extremities and death. In so-called tranquil rabies the animal is in a condition of coma. None of these animals show any aversion to water, contrary to the popular belief.

The symptoms in the human being consist at first of general malaise, mental depression, slight insomnia, and some vague distress about the throat. Attempts at drinking occasion pharyngeal spasm, which spreads to the muscles of respiration, causing a short, quick inspiration like that of a sudden effusion of cold water, this increasing in severity to a strong inspiratory effort, in which the extraordinary muscles of respiration take part, so that the shoulders are raised; and even the facial muscles may draw the angles of the mouth outward. Reflex excitability is enormously increased, the slightest cutaneous impression bringing on a spasm. Salivary secretion is much increased. Vomiting is common. The muscular spasm, confined at first to the muscles of deglutition and respiration, may spread to the other muscles of the body, and the convulsions may become general, causing continuous general muscular rigidity, or opisthotonos, or they may be co-ordinated and closely resemble the hysteroid variety. Delusions may occur, or even wild delirium. The differential diagnosis in the human being is from tetanus, especially when it is endemic, as on Long Island, from that form of organic brain disease known as delirium gravè, from acute mania, from tetanilla, from epilepsy, from hysteria. There is no such dread of water as to warrant the term hydrophobia, as many of even the recent writers would have us suppose; but the spasm of the muscles of

deglutition at the sight of water is only a part of the general reflex excitability, as is shown by the fact that glittering objects, loud noises, draughts of air, may cause the same spasm, or kindred ones, in other muscles. In many cases there is a curious alternation of consciousness with furious action. Thus, a patient begs the bystanders to go away, as he fears he may do them damage; or, as Dr. Ernst tells me that a patient of his did, a man may fly at the throat of an attendant, saying, "I can't help it!" The temperature ranges from 100° to 104° F. or, exceptionally, 106° F. Remissions are frequent. Death is sudden or by gradual coma. Paralysis does not generally occur, although it has been stated that paralytic symptoms similar to those of acute ascending or Landry's paralysis may occur; but as there is only one case to support this claim, and as this one—a man named Goffi—had been inoculated by Pasteur's intensive method, and died notwithstanding, it needs no eminent logician to perceive that the probability of the symptoms having been caused by Pasteur's intensive inoculation is much greater than the probability of their having been caused by rabies.

THE REALITY OF RABIES.*

By CHARLES L. DANA, M.D.,

PROFESSOR OF NERVOUS AND MENTAL DISEASES, NEW YORK POST-GRADUATE MEDICAL SCHOOL.

MR. PRESIDENT: I cannot speak upon the subject of rabies from the standpoint of an experimentalist, and my only apology for accepting the invitation to join in the discussion to-night is that I have been an interested student of the question for a number of years. During the past summer I have visited the Pasteur Institute in Paris, where Messrs. Pasteur and Grancher courteously gave me opportunities of observing the work going on, and of learning their present views upon the subject of the pathology and prophylaxis of rabies. Through the kindness of Mr. Horsley, of London, also, I was furnished with some interesting original documents, as well as a series of photographic studies illustrating the classical and pathological aspects of the disease. M. Paul Gibier, of this city, has also kindly shown me the workings of his laboratory, which, I am bound to say, compare favorably with those of the Paris Institute.

There is still a great deal of scepticism in regard to rabies, its nature, its frequency, and even its existence. There is also a great deal of scepticism and even hostility felt toward the work of Pasteur upon this subject.

In view of this fact it seemed to me that I could not better employ my time in this discussion than in attempting to show what has, in my opinion, been settled in regard to these points.

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[1] Read in part before The New York Academy of Medicine, October 16th, 1890, in a discussion upon Hydrophobia.

I shall, therefore, take up the question of the reality and specific character of rabies in lower animals and in man. This will naturally involve a consideration of the pathological aspects of the disease, and to some extent the principles upon which prophylactic inoculations are based.

The Tests of the Specific and Distinct Character of a Disease.—In order to prove that a certain disease is autonomous, distinct, and special we must establish the fact that its etiology and its clinical history are essentially uniform, or that the anatomical changes found after death are the same, or that inoculations of animals with the secretions or tissues of the victim of the disease reproduce the disease. The proof of the unity of a disease is therefore etiological, clinical, anatomical, and experimental. The unity of some diseases can be established by only one or two of these methods, not by all. In the case of rabies, a comparatively rare and obscure disorder and occurring in lower animals, so that only objective symptoms can be studied, it is imperative that all the proofs should be brought into use, and fortunately this can be done.

I. *The Etiological and Clinical Proof* of the existence of the disease rabies is based upon the fact that different observers in every part of the world, from the time of Cornelius Celsus, Galen, and Aurelian to that of John Hunter (1793), Youatt (1830), and Hertwig (1829), and many distinguished living writers, have all united in describing a disease in the dog having essentially the same origin, symptoms, course, and termination. It is not my purpose to enumerate these distinguishing characters. It is sufficient to say that in them should be included the mode of infection, the period of incubation, the mental excitement, the tendency to bite, the emaciation, acute course, the convulsive and paralytic phenomena, and the invariably fatal termination. But the clinical proof of the existence of rabies is not alone sufficient either to establish it as a specific disease or to make a diagnosis of it

in a particular instance. It has long been known that symptoms resembling rabies may be caused by enteritis, worms, metallic poisons, by farcy, and organic diseases of the central nervous system.

It is true the symptoms when taken in their entirety are generally sufficient for diagnosis. Among two hundred and seven dogs pronounced rabid or probably rabid in Würtemberg in 1865–1866, one hundred and eight, or about fifty per cent., were found to be really so. And Bollinger* estimates this as representing about the accuracy of a professional diagnosis, it being remembered that physicians and veterinaries are always anxious to be upon the safe side.

It follows from the foregoing that those who state that alleged cases of rabies in dogs may be cases of some other disease are stating what no one has ever denied.

Having carefully read the account of Spitzka's experiments† as to this point, it appears to me that he simply showed that symptoms resembling rabies may be caused by meningitis or cerebritis, which no one has denied. Apart from this the experiments have no bearing on the question of rabies at all, and they certainly do not weaken the value of careful experimental tests for rabies made either upon rabbits or dogs.

II. *The Anatomical Proof of the Specific Character of Rabies; Pathological Anatomy of Rabies.*—It is not believed by M. Pasteur and his associates that there are any definite characteristic lesions of rabies.

If any were present they would be found chiefly in the medulla and spinal cord.

As a matter of fact there are found here very uniformly minute changes which consist of accumulations of round cells in the perivascular spaces, forming what has been termed a miliary abscess. These so-called miliary abscesses are most numerous in the later stages of the disease. They may not be found at all in the early stage;

* "Ziemssen's Cyclopædia," iii., p. 469.

† Journal of Comparative Medicine, July, 1886.

and since, in the case of man, the disease usually kills very soon, fewer changes are found in the human nerve centres. In paralytic rabies in man, however, these changes are more decided and widely diffused.* They are always most marked in the medulla, next in the spinal cord, and last in the brain. If patients with rabies lived long enough, we could say, therefore, that anatomically rabies is multiple focal bulbar myelitis. Miliary abscesses, however, are not characteristic of rabies alone, but are found in other diseases, especially infective fevers and meningitis.

A number of other changes, such as softenings, congestions, minute hæmorrhages, etc., have been described as occurring in rabies. Golgi has described peculiar changes in the nuclei of the nerve cells of the medulla. A microbe which can neither be colored nor cultivated can be seen in the brain tissues in rat rabies,† and Babes has described a microbe which Gibier states is identical with the one he has figured. The proof of its specific character is yet wanting. No one has certainly found a microbe, although rabies is probably a microbic disease. And the symptoms of rabies are caused, not by any organic change in the nervous tissues, but by a profoundly irritant poison, the product of the microbe's activity. This poison acts first upon the nerve cells and fibres, and only later does it affect the vascular apparatus.

The anatomical evidence as to the existence of rabies is, therefore, mainly a negative one. By it we can exclude meningitis, encephalitis, hæmorrhages, injuries, etc. There are incidental anatomical changes which help in forming a particular diagnosis, but are not essential parts of the disease. Such are the presence of abnormal substances in the stomach, hyperæmia and hæmorrhagic extravasations in the stomach, congestion of the larynx, fauces, etc.‡

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* Barstow and Horsley : Transactions of the Clinical Society of London, November, 1888.

† Gibier : La Rage, 1884.

‡ The engorgement of the stomach of a dog with foreign indigestible sub-

III. *The Experimental Proof.*—This may be put briefly in this way:

1. The virulence of the poison of rabies was found by Pasteur (as it had been by others) to be chiefly in the medulla, cord, and brain.

2. Animals are made rabid generally by subcutaneous inoculation of this virulent nervous tissue; but they are practically always made rabid by subdural inoculations after trephining.

3. Rabbits inoculated in this way develop a paralytic form of rabies which has a very definite incubation, fifteen to twenty-one days, very characteristic symptoms, course, and termination.

4. This disease in rabbits, called paralytic rabies, is true rabies, because dogs inoculated with its virus develop, after a normal incubation, true rabies.

Thus by establishing the existence and symptomatology of rabbit rabies, and by furnishing a means of always and easily producing it, Pasteur has given us an experimental test of the disease which can be applied with comparative ease. Pasteur experimented still further with the virus of rabbit rabies and reduced it to the most definite conditions. He found that by continued successive serial inoculations of the rabbit the period of incubation after a time was reduced invariably to six or seven days, the rabbits dying on the ninth to eleventh day. He thus fixed the intensity of the virus and the symptomatology of the disease absolutely.

In the Pasteur Institute are ten different cages, each containing an inoculated rabbit. The rabbit in the tenth cage is just dying. That in the first has just been inoculated. Between them one sees rabbits in each stage of

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stances is considered in connection with other symptoms almost pathognomonic of rabies (Law). It is certainly very rare to find such substances in the healthy dog's stomach. In some experiments made by myself (American Journal of Medical Sciences, 1882) on the action of pepsin, I examined the stomach contents of nearly forty dogs and never found foreign matter in them.

paralytic rabies. There is a mathematical precision about the development and course of the disease which affects one with admiration and arouses afresh a confidence that there are laws and certainties in medical science.

In the light of all the evidence thus presented, it seems to me that no one can any longer deny the specific inoculable character of rabies. Further discussion with those who deny it will serve no good purpose. It is, indeed, only in regard to the significance and value of Pasteur's experimental tests that doubts and criticisms still are circulated. Those who distrust Pasteur, however, will find that his work has been abundantly confirmed elsewhere.*

Professor A. Högyes, of Budapest, has recently published a monograph ("Die Experimentale Basis der Antirabischen Schutzimpfungen Pasteur's," Stuttgart, 1889) which includes experimental work patiently carried on for four years, and which entirely confirms the value of this inoculation test of rabies. Dr. Dowdesdell † and Mr. Victor Horsley have repeated and verified the tests. So also have, in Italy, Di Vertia and Zagari,‡ and, in this country, Ernst,§ Shakespeare, and Biggs. There is now, so far as I can learn, universal acceptance of this experimental test by all pathologists who have carefully employed it, or examined the work upon which it is based.

Does the Specific Disease Rabies Occur in Man?—I have spent a good deal of time upon this point, because its establishment is the key to the whole question of the reality of rabies in man, and to all the practical points regarding its prevention. It is unnecessary now to go over at any length the second question, as to the occurrence of genuine rabies (hydrophobia) in man. I do not know that such occurrence is seriously doubted by scientific men

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* The work of Dr. V. Babes is well known. "Stud. über die Wuthkrankheit," Virchow's Archiv, Bd. lx., Hft. 3, p. 562.

† Lancet, 1887.

‡ La Semaine Méd., 1887.

§ American Journal of the Medical Sciences.

anywhere. Yet I am justified in taking up the point, since doubts and denials are expressed not only in certain prominent medical journals, but in discussions on the subject in the lay press. Wide circulation especially has been given to the statement that rabies in men is not a specific inoculable disease,* but a more or less incidental result of bite and fright, of trauma, hysteria, tetanus, meningitis, or insanity.

The proof that a specific inoculable disease known as rabies or hydrophobia affects man is furnished us by the four criteria I have already given, viz.: (1) the etiological; (2) the clinical; (3) the anatomical; (4) the experimental. This last is again conclusive, and puts the fact as much beyond questioning as any point in medical science can be placed. The medulla and cord of men who have died from rabies due to the bite of a dog, have been inoculated subdurally in rabbits, and paralytic rabies is produced, with only a difference in the period of inoculation. The course and termination are identical with those caused by inoculating rabbits with the virus from a rabid dog. These experiments have been done by different observers in different parts of the world. The accounts of them can be read in the communications made by Pasteur and others in the *Annales de l'Institut Pasteur*, in the report of the British Parliament Committee in 1877, and in the Transactions of the London Clinical Society, November, 1888, and in numerous other papers.

Many doubt the existence of human rabies because they have never, in a long experience, seen a case. Rabies in man is undoubtedly a rare disease in this part of the country, but cases are constantly occurring here and there. Within six months two cases were brought to Bellevue Hospital. The frequency of the disease in general is not to be estimated by its occurrence in America. Its prevalence in Europe is very considerable, as has been shown by the records collected by Bollinger before Pasteurism began, and by later writers.

* Dulles: Medical Record, 1877, p. 672.

Pasteur's work has called attention also to the fact that a paralytic form of rabies sometimes affects man, producing symptoms resembling acute ascending paralysis, or Landry's paralysis. It is not impossible that cases of this disease have been in reality of rabietic origin. In questioning the many general practitioners from various parts of the country with whom I come in contact, I have found that many could recall cases of mysterious acute, progressive, fatal paralysis, whose nature and cause have completely puzzled them. It may be that the paralytic rabies in man is, therefore, not such an extreme rarity. Gamaleia,* Ygouff,† Ufridizzi, Bristowe and Horsley,‡ and Surgeon-Major Keegan, have all reported cases of paralytic rabies in man.

In the attempt to discredit Pasteur and disprove the existence of human rabies, a great deal has been made of pseudo-hydrophobia, or lyssaphobia, and of its alarming frequency and extraordinary dangers. All the old-wives' tales illustrating the power of the imagination to disorder and annihilate the body have been rhetorically rehearsed, and a fear of hydrophobia has been supplanted by a fear of the fear of hydrophobia, so that good people are shivering in terror over an innocuous shadow while they smile incredulously at the fatal substance.

As a matter of fact there are no authentic clinical records of a single case in which fear of hydrophobia has caused a disease measurably similar to rabies, by which I mean a disease not easily distinguished from rabies by an intelligent physician.§ Nor is there, so far as I can find, a single authenticated case of death from this particularly hypothetical phantasm.‖

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* Vide Ann. de l'Institut Pasteur.

† Thèse de Paris.

‡ Transactions of the London Chirurgical Society, 1888.

§ Dr. Ernst's case of spurious rabies, cited at the discussion of this subject, exactly proves this point.

‖ Romberg cites a fatal case of lyssaphobia from Trollert, but there is no evidence that the trouble was not either true rabies or acute alcoholism, since the symptoms developed directly after a spree and three and a half months

The imagination alone cannot fatally inhibit the vital functions; the *psyche* cannot suddenly destroy itself by creations of the imagination, any more than the *soma* can without help commit *auto da fé.*

The legend of the man who died because he was told that he was being bled to death, while, in fact, a stream of warm water was allowed to trickle along his arm, ought to be laid away with the ancient and inane myths. It is not historically authenticated, and it is psychologically impossible. Bear in mind that I am speaking of the effect of imagination upon healthy bodies. No doubt fright can kill a person who is already toppling on the verge of the grave, or who has a weak heart or but one lung, or who is ripe for an insanity. But as for hydrophobic fright inducing an acute and fatal disease in strong men or healthy women, it is inconceivable.*

Prophylaxis.—Closely connected with the subject of the specific character of rabies in men and the lower animals, comes the question of the production of immunity from its horrible and fatal effects. I shall leave any elaborate argument on this question to others, but I cannot forbear bringing up some points, since they are so intimately woven in with the line of argument I am trying to pursue. The establishment of the specific inoculable character of a disease gives a presumption in favor of the power of

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after a bite. Another very ancient and doubtful case of fatal lyssaphobia is reported in the memoirs of the Royal Society of Montpellier. It is curious that modern literature furnishes no fatal cases, and that those who speak so fluently of it have never reported a case.

* There have been, perhaps, fatal cases of tetanus following bites of dogs, and there have possibly been fatal cases of acute mania generated in the predisposed by the fright. These extremely rare and doubtful instances will explain the fatal cases of so-called pseudo-hydrophobia.

It is but just to say that in the discussion on this question the distinguished chairman asserted that, as one result of the discussion, it had been proved that there was such a thing as false hydrophobia, and that this disease had been fatal.

I did not deny that false hydrophobia occurred, but only that it was not like true hydrophobia, and was easily distinguished from it. No one reported an authentic case of fatal lyssaphobia.

securing immunity from it, for in the great majority of specific diseases the animal body either naturally possesses or may acquire immunity. This is true of all, or nearly all, the non-suppurative infections, from syphilis to small-pox. As regards animals, it seems to me that no one who carefully reads the evidence given in Pasteur's communications can deny that by his method he succeeds in rendering dogs refractory to the bite of other rabid dogs, also to subcutaneous and even to subdural inoculations of the virus of street rabies and of the "fixed virus"; also that, even after subcutaneous inoculations of this virus or after infecting bites, he can prevent the development of the disease. His experiments have been reported in detail, and have been followed by such experienced laboratory workers as Lauder Brunton, Mr. Horsley, and Dr. Burdon Sanderson.

Besides these they have been repeated and confirmed by Ernst, Babes, Högyes, Bujwid, Di Vestia, and others.

Much has been made of the experiments of Von Frisch, of Vienna, who alone, so far as I know, failed to get the same results as Pasteur. But his failure is easily explained. The rabbits used by him were smaller and the cords lost their infective properties too soon, while he himself failed to operate aseptically. Instead of continuing his experiments with the desire to eliminate these sources of error, Frisch stopped his work and announced, much to the satisfaction apparently of the Viennese world, that the method was a failure. And Billroth said, "French science is advancing with a clubfoot," and only two papers out of nearly a hundred were read by Frenchmen in the Section of Internal Medicine at the Berlin International Congress.

Now, Prof. Högyes had at first the same experience as Frisch, but he continued his work and finally succeeded in making the dogs refractory to rabies.

It may be admitted that the process is not always certain or absolutely perfect. Of this others can speak with much more authority than myself; but the essential prin-

ciple has been established as immutably as anything in biological science.

Thus it seems to me, in conclusion, that the following points are established with regard to rabies:

1. Rabies is a specific inoculable disease. It occurs not only in lower animals but in man; and the etiological, clinical, anatomical, and experimental tests all unite in proving that rabies is the same specific disease in whatever animal it occurs.

2. The experimental test of the disease by subdural inoculation of rabbits with nervous tissue is a certain method (barring the natural accidents of surgical experimentation) of determining the existence of rabies in a given case.

3. The most constant anatomical change in rabies is a multiple focal bulbo-myelitis. This condition is found in man if life is prolonged.

4. The paralytic form of rabies may occur in man, assuming somewhat the type of Landry's paralysis.

5. The virus of rabies resides chiefly in the nervous centres, and can be rendered definite in action by carrying it through rabbits.

6. Dogs and other animals can be rendered refractory to rabies by protective inoculations.

7. Spurious hydrophobia is rare, non-fatal, and easily distinguished from true rabies.

THE PRESENT EXPERIMENTAL ASPECT OF PASTEUR'S PROPHYLAXIS FOR RABIES.*

By HERMANN M. BIGGS, M.D.

THE discoveries of Pasteur regarding the causation and prevention of rabies are of the highest scientific importance and form very valuable contributions to our knowledge of this disease, but still the subject remains as yet in an unsatisfactory condition.

Almost five years ago an account of the method pursued in the treatment was published, and the inoculation of human beings was begun. The system in the beginning was purely experimental and empirical, and depended entirely for its confirmation on the practical results obtained by its application. The nature and seat of the virus, the method and cause of its attenuation, the manner of employment, and the effect produced, were all experimental questions, and the system was without other than an experimental foundation.

During the last five years more than twelve thousand persons have been subjected to the treatment, to say nothing of the experiments on animals; but, unfortunately, even now we have scarcely more accurate knowledge of the principles underlying the method and its action than in the beginning. The cause of the disease, although probably rightly assumed to be a micro-organism, remains yet entirely unknown, the observations on this point thus far made being without confirmation; and, judging from the report of different observers, protection from rabies may be granted to human beings, after

* Read before The New York Academy of Medicine, October 16th, 1890.

the bites of rabid dogs, by methods widely divergent in character and quite unlike that of Pasteur—in fact, by the use of such virus as one would be led to believe, from Pasteur's statements and observation, must certainly result in the production of the disease. Although the results of almost all observers have tended to confirm in all essential particulars Pasteur's conclusions, yet the apparent enormous increase in the number of cases of rabies that followed Pasteur's announcement, and that has followed everywhere the establishment of anti-rabic institutes, has tended to throw discredit in many minds upon the method and the conclusions drawn from its application as to its practical value so far as human beings are concerned.

So much has been written during the last few years concerning the method of inoculation that it will not be necessary to detail it here, and, in fact, the time allotted to the experimental aspect of the rabies question would not permit of its consideration. We shall, therefore, take up at once a review of the reports of the recent experimental investigations in rabies.

The most important fact in the recent history of rabies is that all of Pasteur's statements concerning the procedure of rabies vaccination, and its efficacy in experiments on animals, have been in all essential particulars unreservedly corroborated by nearly all authors of the last two years. Högyes,* Bujwid,† Pecirka,‡ and Celli and De Blasi, all emphasize in their publications the agreement of the results obtained in their numerous personal investigations with those of Pasteur. These authors also admit the applicability and value of the method in man.

A number of investigators have made efforts to simplify the method of Pasteur, to make it more certain and less dangerous. Here may be mentioned the treatises of

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* Annales de l'Inst. Pasteur, 1888, No. 3, p. 133.

† Centralb. für Bacteriol. u. Parasitenk., Bd. 3, 1888, No. 25, p. 798.

‡ Sbornik, Lekarsky, t. 2, fasc. 3, p. 307.

Babes,* Galtier,† Nocard and Roux,‡ Protopopoff,§ Roux,‖ Ferran.¶

Babes recommends a mixture of spinal-cord substance of different strengths for preparing an effective series of vaccinating material, and claims to have succeeded with this with far more certainty than with Pasteur's method in protecting dogs against rabies, even after subdural infection with the virus of street rabies.

Galtier has taken up again his experiments begun in 1880–81, which showed that immunity against rabies could be produced in herbivorous and omnivorous animals by an intravenous injection of the virus, and he has confirmed the results he had formerly obtained. He succeeded with certainty by this method (one or two intravenous injections of an emulsion of the oblongata of mad dogs) in producing immunity to rabic virus in animals (sheep, goats, pigs) bitten by rabid dogs. Nocard and Roux confirm these statements of Galtier, but they show that immunity can in this way be gained only against the poison of street rabies and not against the *virus fixé.*

Protopopoff has found that dogs also can be made immune by the method of intravenous injection, even against the most potential effect of rabic infection, that is, subdural vaccination with *virus fixé.* For this purpose there are not required so many preliminary vaccinations as in the subcutaneous method of vaccination of Pasteur, as only three injections (emulsions of six-, three-, and one-day-old spinal-cord substance) are sufficient to effect immunity.

The statements of Protopopoff were later corroborated by Roux, and he refers to some old experiments at the

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* Annales de l'Inst. Pasteur, 1888, No. 3, p. 133.

† Comptes rendus de l'Acad. des Sciences de Paris, 1888.

‡ Annales de l'Inst. Pasteur, 1888, No. 7, p. 341.

§ Cent. für Bacteriol. u. Parasitenk., Bd. 4, No. 3, p. 89, and No. 25, p. 787.

‖ Ibid. (1), 1888, No. 1, p. 18.

¶ Ibid. (1), 1886, No. 2, p. 97, and Gaceta Medica Catalana, t. ii., 1888, No. 4.

Pasteur Institute which showed that dogs at times can be made refractory even after a single intravenous injection of rather weak rabic virus.

The efforts of Ferran have also been directed to the discovery of a shortened method of inoculation. He believes that the inoculations may from the beginning be made with most virulent rabic virus—that which has acclimatized itself in the body of the rabbit. Without first testing the method on animals, he at once applied it to man, and fortunately with success.

Di Vestia and Zagari, and others, have shown that the virus extends from the seat of infection along the nerve trunks to the spinal cord, and if animals are killed at the proper time the virus may be shown to exist in the nerves leading from the part infected, when it is as yet absent in other parts of the nervous system. So in bites of the hind extremities it is present in the lumbar cord when absent in the cervical region or medulla.

It has been further shown that susceptibility to the virus varies with the kind of tissue into which the injection is made. Intravenous injections are not often followed by the disease. Injections into the muscular tissue are more often followed by infection than injection into the subcutaneous tissue, and the application of the virus to divided nerve filaments is generally efficacious in producing the disease. Exposure to comparatively low temperature suffices to destroy the virulence of rabic virus, *i.e.*, exposure for one hour to a temperature of 50° C. (122° F.). Drying, the direct rays of the sun, and exposure to action of one per cent. sublimate or of one per cent. carbolic solution for three hours, has the same effect. It may be preserved by immersion in pure glycerin, or, as Dr. Byron has shown, by sealing up aseptic pieces of cord in test tubes. By the latter method the virus may be preserved in all its virulence almost indefinitely.

As has been repeatedly shown, the virus is found most abundantly in the central nervous system, and especially in the medulla. It is found also constantly in the salivary

and lachrymal glands, and sometimes in the pancreas, but is usually absent from the blood, liver, spleen, and kidneys. It is rarely present in the secretion of the mammary gland, and may occasionally be transmitted to the fœtus through the placenta.

Ferran's observations, if confirmed, are very important. He uses in his inoculations what he calls the superintensive method. He says that each person inoculated receives practically an amount of emulsion equivalent to a whole rabbit's brain—twenty times the virulence of Pasteur's inoculations. This virus is prepared by mingling twenty-five cubic centimetres of water in a mortar with ten grammes of sterilized sand and a rabbit's brain entire. After triturating well he allows the emulsion to stand ten or fifteen minutes, so that only the sand sinks to the bottom, and then fills the syringe with the emulsion at about half its depth, where there is no sand but much disintegrated cellular detritus, and where the emulsion is rich in virulent matter. The emulsion is very fine, thick, pink in color, and neutral in reaction. The emulsion is only used when fresh. The serial inoculation of rabbits is made by scarifying the cornea instead of by the subdural injection of Pasteur. The rabbits die in the same way as after Pasteur's system, only that the incubation period is on the average a day or two longer.

In human subjects the virus is deposited by Ferran with great care in the cellular tissue. No symptoms are produced, either local or general. Every person receives from the first day two cubic centimetres of virulent virus morning and evening for five days, *i.e.*, they receive twenty cubic centimetres of the most virulent virus.

No variation is made either for the age of the patient or the kind of bite; all have the same treatment. In the beginning Ferran used forty cubic centimetres of virus (four cubic centimetres daily for ten days). Many dogs have been successfully inoculated as a prophylactic measure by the same method as that used in the human being. In

more than one hundred cases immunity has been conferred upon dogs in this manner.

The peculiarities of Ferran's system consist in:

1. Serial cultivation of virus by corneal scarification.

2. Employment of one vaccine, and its use for all people without reference to the gravity of the bites.

3. Substitution of all serial attenuated cords by the virus from one single fresh brain.

4. Reduction of treatment to five days and twenty injections.

Ferran says that immunity grows proportionately to the quantity of virulent virus employed. Man will tolerate easily four cubic centimetres of virulent virus daily, although this is sufficient to kill a rabbit.

In the simple method of Pasteur's system cords were used from the fifteenth to the fifth day, and of these only the last four at least are active. In the intensive method of Pasteur, which superseded the simple, three cubic centimetres of virus were inoculated daily for ten days, and of this whole amount employed only three cubic centimetres were inactive. In other words, in the simple method three or four cubic centimetres in all of the active virus were introduced, and in the intensive method twenty-seven cubic centimetres were introduced, during the same period. The modified intensive method now employed uses twenty-two cubic centimetres for grave cases (first class); seventeen cubic centimetres for less grave cases (second class); eleven cubic centimetres for doubtful cases (third class).

Ferran calls attention to the greatly diminished death rate in all Pasteur institutes since the employment of the intensive method. This is certainly a fact, and is not what one would be led to expect.

Gamaleis had seven per cent. of deaths by the simple method, and only about one and a half per cent. by the intensive.

As to the superintensive method of Ferran, it may be noted that Bareggi, after trying Ferran's method with

apparent success on animals, inoculated five human beings as he supposed according to it. They all died within a short period from paralytic rabies, and his institute was closed by the government authorities.

Ferran insists so strongly that immunity grows proportionately to the amount of virulent virus employed in the inoculation, that he makes his patients swear to finish the treatment if they once begin it. He says that Bareggi's misfortune was due to the small amount of virus used. From May 10th, 1887, to October 31st, 1889, Ferran inoculated by his method six hundred and fifty-two patients, with three deaths. It should perhaps be said that Ferran's statements may be accepted with considerable reservation in view of his experience with cholera.

Högyes has described another method for the conference of immunity. He made a series of inoculations, in a dog, of one cubic centimetre of emulsion of fresh cord every two hours, of the following proportions of cord to water: 1st, 1 : 5,000; 2d, 1 : 2,000; 3d, 1 : 500; 4th, 1 : 250; 5th, 1 : 100; 6th, 1 : 10. These were repeated on three successive days. Five days after completion of the first series, the series were repeated again on three successive days, and five days later they were again repeated for three days. No symptoms had been produced fourteen days after the last inoculation, and he then inoculated the dog thus protected with the cord of a rabbit dead with a nine-day rabies. The control dog died after fourteen days, the one protected remained well. Then on May 26th he inoculated three other dogs by the same method. On July 10th these were caused to be bitten by a dog suffering from street rabies, and none died. Four months later they were inoculated under the dura with a virulent cord. They all resisted, while the controls died. Johne, in a review of the subject written in 1889 for Baumgarten's *Jahresbericht*, says: "The state of the important question of rabies has not changed since last year to any material extent. The microbe of rabies has not been discovered, and the veil of doubt which has surrounded the

question of the practical value of Pasteur's method of vaccination has not been lifted by the later investigations and observations. Even in the face of the latest statistic proofs we are not prepared by any means to dismiss the scruples we have formerly expressed as to its applicability in the human being."

That we have yet much to learn about rabies is pretty clearly shown by the vast difference in the methods that are used with approximately the same results. In Pasteur's simple method three or four cubic centimetres of active virus, the strongest five days old, were used in the entire treatment; in virulence probably the whole amount employed was not equivalent to more than one cubic centimetre of the fresh medulla, and was used only after tolerance had been established by the use of a series of cords possessing a gradually increasing virulence, commencing with one having no appreciable activity. In the intensive method the virus employed exceeds this by ten times in both virulence and amount. In Ferran's method, without any preliminary inoculations with weak virus, immediately the fresh, most virulent virus is employed, not one centimetre daily, but four centimetres daily; and these inoculations are repeated on five successive days, and were formerly repeated on ten successive days. According to Ferran, the more the virulence and the greater the quantity of virus introduced, the greater the immunity; and this immunity is immediately acquired, apparently, for he commences with inoculations of virus far exceeding in virulence and amount that which could be introduced by any possibility through the bite of a rabid dog. On the other hand, Högyes produces immunity by the frequent injection of very small quantities of virus.

Without further comment on this aspect of the subject, I shall append here the statistics of the various Pasteur Institutes, and, without reading them, will merely say that the mortality has varied from nearly seven per cent. to less than one per cent. in the Pasteur Institute in Paris during the last year.

STATISTIC DATA OF INOCULATIONS AT ANTI-RABIC INSTITUTES.

Pasteur's Institute.—Up to January 1st, 1887, 2,682 inoculated, 35 died of rabies; during the year 1887, 1,646 inoculated, 29 died of rabies; during the year 1888, 1,616 inoculated, 19 died of rabies; during first half of 1890, 1,049 inoculated, 9 died of rabies.

Anti-rabic Laboratory of Turin.—531 inoculated, 10 died of rabies.

Laboratory of Palermo.—292 inoculated, 2 died of rabies.

Laboratory of Odessa.—Inoculated by the simple method, 324, mortality 3.39 per cent.; inoculated by intensive method, 1887, 345, mortality 0.58 per cent.; inoculated by intensive method, 1888, 364, mortality 0.64 per cent.

Laboratory of Moscow.—Inoculated by simple method, 107, mortality 8.40 per cent.; inoculated by intensive method in 1887, 280, mortality 1.27 per cent.; inoculated by intensive method in 1888, 246, mortality 1.60 per cent.

Laboratory of St. Petersburg.—Inoculated, 484; mortality, 2.68 per cent.

Laboratory of Varsovia.—Inoculated, 676; died of rabies, 9.

Laboratory of Havana.—Inoculated, 121; died of rabies, 1.

Laboratory of Buenos Ayres.—Inoculated, 286; died of rabies, 2.

Ferran's Laboratory.—Inoculated to October 31st, 1889, 652; died of rabies, 3.

Reference should be made to several cases published in the *Annales de l'Institut Pasteur* in 1889, in which two or more persons having been bitten by the same dog, those who were inoculated remained well, and those who did not undergo treatment died of hydrophobia. Among the most remarkable of these cases were the following: On June 1st and 2d, 1889, eight persons from Vacluse were bitten by the same dog. Six of them submitted themselves to the anti-rabic treatment and are now quite

well. Two declined to be inoculated, and both died of rabies, one on July 1st, the other on July 2d. Pierre Butt and his wife were licked by a rabid dog on open wounds. The wife declined to be inoculated and died of rabies. Butt, on the other hand, was inoculated and is now quite well. In an address by Dr. Ruffer a letter is quoted from Prof. Babes, of Bucharest, giving particulars as to the results of the Pasteur treatment in that city. From May 1st to August 1st two hundred and forty-four persons were inoculated after having been bitten by rabid domestic animals. The mortality among these persons was *nil.* On the other hand, thirty-nine persons bitten by the same rabid animals declined to undergo treatment, and four of these at least have been certified to have died of rabies. Thirty-four persons were badly bitten on the hand by wolves, and were inoculated, and of these three died of rabies. On the other hand, every one of the persons who refused to undergo treatment, after having been bitten by the same wolves as those before mentioned, has died of rabies, as have one hundred and eighty head of cattle bitten by the same animals. The following facts are given on the authority of Dr. Högyes: Between November 1st, 1885, and the end of June, 1888, five hundred and thirty-two human beings were officially registered in Hungary as having been bitten by rabid animals. Sixty-two of these persons were inoculated, and not one has died of rabies. On the other hand, of the four hundred and seventy persons who were not inoculated, forty-four at least have been certified as having died of rabies.

From the purely experimental side of the question, it seems to me that the evidence is very strong of the protective influence of Pasteur's inoculations in both animals and man. In fact, it appears to me that an unprejudiced observer must assume either that there is no such disease as rabies, that Pasteur and other observers are not dealing with rabies, or must candidly admit that the inoculations do give relative immunity to rabies.

As to the absolute correctness of Pasteur's statistics,

and the relative mortality after the inoculations, I confess to the greatest incredulity.

First. Because I can find no evidence to justify the assumption that there are anything like as many cases of rabies in France, or anywhere else, as one would be led to suppose there were from the number of patients inoculated in Paris and in other anti-rabic institutes.

Second. It seems to me, from personal experience in sending patients to the Pasteur Institute, that too little care is used to determine whether the persons presenting themselves for inoculation have been bitten by rabid dogs, and no attempt is made to follow the history of some (foreign) cases, at least, afterward. It can only follow that Pasteur does not know, in these instances at least, whether the patients remain well or die of rabies after they have left his care.

I have sent during the last few years a number of patients to Paris to be inoculated, and during the last winter sent two children, bitten by a dog proven to be rabid, who were watched during their treatment by my brother, Dr. Chauncey P. Biggs. No attempt has been made in any of the cases, so far as I can determine, to either obtain a careful history of the cases or to follow them afterwards. To my certain knowledge some cases have gone from America (not at my suggestion), and have been inoculated, where there was not the slightest evidence of their having been bitten by rabid dogs. I will interpolate here an account of the last cases sent, as they give the clearest history of rabies of any that have come under my observation, and present some features of interest.

A stray black bitch, we will call dog No. 1, bit Miss H.'s black spaniel (No. 2) at 14th street and Fifth avenue, about November 15th, and ran on down the street. On Thursday, December 6th, the spaniel began to be very restless and uneasy, showed constant thirst, did not eat, and behaved queerly, though not attempting to bite any one, and obeying his name when called. On Saturday, December 8th, he suddenly turned on and rolled over Mrs.

H.'s little Dachshund bitch (No. 3). He was then isolated, and died during the night of Sunday, December 9th. Everything in the room where he died was much torn and gnawed. This dog was sent to me for examination at the suggestion of Dr. Beverley Robinson, and the succeeding experiments were carried on by Dr. Byron and myself. The autopsy on this dog (No. 2) showed the indefinite lesions commonly found in rabies cases, and the absence of any sufficient anatomical cause for death. Two rabbits were inoculated under the dura, in the usual way, from the medulla of this dog, on December 10th. These rabbits died of characteristic dumb rabies on December 28th.

Dog No. 3 was rolled over by dog No. 2 on December 8th. On December 21st she became restless, and, gradually growing worse, died with the symptoms of rabies on December 29th. (I would call attention to the long illness—eight days—in this case and its possible relation to the small quantity of virus received, as no bite could be found on the animal.)

The autopsy on dog No. 3 reveals the same condition of organs as in dog No. 2. Rabbits were inoculated with the medulla from No. 3 in the usual way; one of them died from the inoculation, but the other remained well until about the fourteenth day, when it began to exhibit symptoms of dumb rabies and died two or three days later. Dog No. 3 bit two other dogs and two children. In the case of one of the dogs, the owner poured some tincture of iodine into the wound at once. Both of these dogs were taken sick fifteen days later, in the same way as the previous ones. The first one, whose bite had been treated with tincture of iodine, after four or five days gradually recovered, and the second one (dog No. 4) died with the usual symptoms of rabies. Two rabbits were inoculated from this dog; one died of dumb rabies and the other remained well. The two children were both bitten on the hand on December 24th. The bites were not very serious, although a good deal of skin was scratched off by the animal's teeth. The bites were not cauterized until two days later. As there seemed to be no question but that the dog which bit them was suffering from rabies, they were sent to Paris, sailing on January 1st, and received their first inoculation on January 11th, seventeen days after receiving the bites. In these cases four series of dogs, and three couplets of rabbits inoculated from the

dogs, all died of rabies, with the exception of one rabbit and one dog (the one that had iodine poured at once into the bite) which developed rabies, but finally recovered. The children were inoculated and also escaped.

Let us see now exactly what foundation there is for a belief in Pasteur's conclusions.

His first conclusion is that the virus of rabies is present in a concentrated form in the central nervous system, and especially the medulla and the spinal cord. This observation was made by Duboisia in 1879, and has been confirmed by all competent observers since that time. My own observations entirely confirm this conclusion.

His second conclusion is that rabies may be produced with the greatest certainty by the subdural inoculation of other animals with portions of the brain and spinal cord of animals dead of rabies.

This claim has also been repeatedly confirmed, and during the last year has been verified, in at least half a dozen cases of street rabies, by the experiments of Dr. Byron and myself, one series of which has been detailed. The proof in favor of this is overwhelming, and familiarity with the facts must compel its acceptation.

The third conclusion is that the virulence of the virus can be increased and the period of incubation shortened by the successive subdural inoculation of rabbits. All the series of experiments made to verify this claim, so far as I know, have entirely confirmed it.

The fourth conclusion is that the rabic virus present in the brain and spinal cord may be attenuated in a constant and progressive degree by drying the cord at a fixed temperature, and that the virulence is entirely destroyed after about fourteen days. This observation has been repeatedly confirmed and needs no discussion. Up to this point there can be no question about the complete acceptance of the conclusions of Pasteur.

The most important conclusions follow; the first of these is, that by a series of inoculations with a rabic virus attenuated by drying, but of a gradually increasing viru-

lence, he had been able to finally render dogs and rabbits refractory to inoculations with the most virulent virus, and to protect them from the disease after infection with fresh street rabies. All of the more recent investigations seem to confirm this statement, almost without reservation.

This brings us to the consideration of the most important question—namely, that this prophylactic method is equally applicable to the human being, and that persons bitten by rabid dogs can with certainty be protected from hydrophobia if they are subjected to the treatment within ten days of the bite, or before the period of incubation has elapsed. This is the practical question in the method.

The first point that impresses one, on carefully reviewing the subject, is the enormous number of cases of rabies that suddenly appeared after Pasteur's announcement, especially in France, and later in all parts of the world where anti-rabic institutes have been founded.

Until recently rabies and hydrophobia have been considered very rare diseases in all countries, but in one year after the announcement of this discovery nearly two thousand Frenchmen were subjected to this treatment because they were supposed to have been bitten by rabid dogs and were in imminent danger of hydrophobia. This astonishing fact at once attracts and holds our attention. If these patients were in reality bitten by rabid dogs, the disease can no longer be considered a rare one, but must have become a very common one. The most careful and reliable estimates—those of Leblanc—as to the proportion of persons, bitten by rabid dogs, who develop hydrophobia, place the ratio at sixteen to one hundred. Bouley, however, says that the proportion fixed by Hunter—*i.e.*, five to one hundred—approaches nearer the truth. According to the statements of Kischenski, among three hundred and seven persons bitten by unquestionably rabid dogs, there were seven deaths, a mortality of only a little over two per cent. If we accept the ratio of

sixteen to one hundred, as Pasteur has done, there would have been three hundred and twenty deaths from hydrophobia in France in 1886. This is what Pasteur alleges would have occurred. Let us glance now at the statistics of this disease for the last few years in France and other European countries, and see how they compare with such an estimate of the death rate in France for 1886. Boudin, in a communication to the French Academy in 1863, gave the average death rate from hydrophobia in Prussia from 1854 to 1858 as 19.5; in Bavaria, from 1855 to 1856, as 3.5; in Belgium, from 1856 to 1860, as 2.6; in England, from 1853 to 1857, as 10; in Scotland, from 1855 to 1863, as 1; in Sweden, from 1856 to 1860, as 4.2.

In Austria the deaths averaged eight yearly for the six years ending in 1885. In Prussia the death rate during the five years were ten, six, four, one, none respectively, ending 1886, or 4.2 annually. In Holland the average was 2.1 for the ten years ending in 1879, and during the last five years no deaths have been recorded in the official government reports. In 1887 in the whole German Empire there were only four deaths from rabies. In France, according to the reports of Tardieu and Brouardel, for the years 1850 to 1872, there were in all six hundred and eighty-five deaths, or an average of a little less than thirty yearly. Tardieu says of his reports that nearly all the departments replied to his inquiries, and Brouardel that about two-thirds were included in his.

In New York City during the last ten years there have been in all nine deaths reported to the Health Department as being due to rabies. (The New York statistics were kindly furnished me by Dr. Roger S. Tracy.)

In 1883 it is reported that one hundred and eighty-three animals suffered from rabies in France; in 1888 this number had increased to eight hundred and sixty-three. Statistics, to be sure, are not worth much, but when the statistics of all countries, as well as the consensus of medical opinion everywhere, show that rabies has been a rare disease, I cannot understand why one should accept with-

out questioning the statement that three or four hundred lives are saved annually in France, as many more in Russia, and a proportionate number in other countries where there are anti-rabic institutes.

In Belgium, Holland, Germany, Austria, Norway, and Sweden there have been no institutes, and there is scarcely any rabies.

The unfortunate influence which the Pasteur institutes have everywhere had in increasing the apparent number of cases of rabies, is not to be charged to the system itself, but to the sensational reports which have been published constantly in the daily press. This procedure can hardly fail to have the effect which has been produced. The history in America in this respect has not differed from that in Europe.

Most of the recent defenders of Pasteur's statistics base their arguments as to the efficacy of the method on those cases in which the dogs by whom the patients were bitten have been certified to be rabid by veterinarians, or in which the dogs were proven to be rabid by inoculation. The number of cases in these classes is very large and the percentage of deaths small. However this may be, the fact still remains that whereas, before Pasteur's system was introduced, there were, so far as can be determined, few deaths from rabies—forty to fifty annually in France—now there are from twelve hundred to two thousand Frenchmen inoculated yearly. The average death rate in Pasteur's statistics must be small. The only explanation that commends itself to my mind, as to the great number of cases inoculated, is that many of these have not been bitten by rabid dogs. I do not believe that the certificate of a veterinarian, of the existence of rabies in a dog, or the proof to be derived by inoculations when these inoculations are made by men unaccustomed to experimental work, are to be considered of great value in determining the presence of the disease in suspected animals.

However this may be, even if the percentage of mor-

ality in human beings should be really larger than it apparently is, still the evidence of the protective influence of the inoculation, even in human beings, cannot be doubted.

In conclusion, then, we may say that experimentally, in animals, Pasteur's method grants relative immunity to rabies, and probably protects, in part at least, the human being, although the inoculations be made after infection.

HYDROPHOBIA.*

By HAROLD C. ERNST, M.D.,

BOSTON, MASS.

Paper not prepared for publication.

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* Remarks made in the discussion upon Hydrophobia before The New York Academy of Medicine, October 16th, 1890.

THE AMERICANS AND THE TENTH INTERNATIONAL CONGRESS.*

By A. JACOBI, M.D.,

NEW YORK.

On August 4th, 1890, during the first, and largest, general meeting of the Tenth International Congress, there were three universal and spontaneous outbursts of applause. The first and most sympathetic greeted the name of James Paget, and never was there an ovation more deserved. The second rang through the immense building when it was announced that the government of the French Republic had sent thirty-four official delegates, and that nearly one hundred and fifty more Frenchmen had joined the Congress. They had overcome political enmity and jealousy, disregarded a rather slighting reference to their "national insanity" of twenty years ago, and came with open hearts and friendly feelings, a large number of them men of fame and high rank. The third greeted the announcement of the fact that on the first day of the gathering more than six hundred Americans were inscribed on the rolls. This recognition afforded to our name must have flattered the national pride of every one of us who was present.

This hearty welcome was more than I had mustered the courage to expect. For, indeed, Americans visiting Europe on such occasions as this labor under certain difficulties. Europeans do not quite understand our country, its political and social configuration or its scien-

* Being an introduction to special reports of Delegates to the Congress before The New York Academy of Medicine, November 6th, 1890.

tific attainments. If that be so even in Great Britain, both race and language being identical and mutual intercourse more frequent, how much less can we expect it to be known on the Continent! Besides, it is not always the best political, social, and scientific class of our fellow-citizens who travel extensively, and, though it be not the crowd of the "profanum vulgus" that ought to tell in the estimation of the best spirit of their country, it does so tell. Now, the majority of medical Americans they know in Europe, and particularly in Germany, belong to one of two classes: either they are *bona fide* students, whom, being mere foreigners, they consent to matriculate even without the preliminary education rigorously insisted upon in their own young countrymen, or they are our young doctors who pass a few months or a year in European laboratories and clinics for the sake of special studies. It is these latter that are also the occasional participants in their national associations, where, nobody else being present, they are naturally considered the representatives of American medicine. Our best men travel little and talk less. Indeed, some of those who were most fit to represent us in the Congress kept in the rear, modest and retiring. Besides, the great opportunity America might have had to present to the view of the world whatever there is great and progressive in American medicine appears lost. For in the very number of the *German Medical Weekly* which was published in the week of the Congress, you could, in the history of previous congresses, read the statement that the Washington Congress was unfortunately a failure, for which all of us, being Americans, are held responsible. Moreover, though English is read by a great many of the best men in Europe, the knowledge of our language is not so general as to insure a wide acquaintance with our literature through anything but the uncertain channels of extracts or translations. Nor are even these well selected. We are all aware that our medical journals are of as unequal rank as our schools, and not infrequently will you find

a journal which is deservedly unknown among us quoted in Europe under the impression that it is a fair representative of American medical literature. Nor is the treatment Europeans receive at our hands always very courteous or considerate. The editorial remarks of a great New York weekly were quoted as unkind, inasmuch as the efforts to make the Congress international and Berlin a neutral ground for the whole world did not appear to be appreciated by us. It must be admitted, though, they did not deem that Western journal worthy of serious consideration which spoke of the Tenth International Congress as a congress of snobs, and advised every one of the forty thousand practitioners of the Mississippi Valley, "every one superior to the leaders of the Congress," to stay at home.

Public opinion is often made or unmade by trivialities; sometimes, indeed, by personalities of an inferior nature. It was a source of complaint in Berlin that an American who had been honored with the request to represent our country by delivering one of the great addresses, had neglected to see to it that his refusal reached the Committee of Organization in anything like due time. The proverbial courtesy of Americans was found wanting, and that at a time of feverish excitement and overwork. Such occasions are the very opportunities for those, formerly Europeans, who manage to rise, in their own estimation and that of their former countrymen, by detraction of us. For there are those who do not immediately succeed, when they, our guests and future fellow-citizens, arrive among us, in impressing us with their superiority, or in being appreciated by us as they are by themselves, or in obtaining at once a lucrative practice and professional positions and honors. It is they who pay for the hospitality, proffered by our country, with shoulder-shrugging insinuations and pitying remarks upon our crudeness and inferiority, our "mob rule," our "civilized barbarism," instead of aiding in the realizations of the

national and cosmopolitan aims of the medical profession and science.

Nothing is so small as not to have some effect. Unfortunately, there is still so much national jealousy everywhere that faults and shortcomings in your neighbor beyond the boundary line are easily believed in, and slanderers and libellers are always busy. When I arrived in Germany a newspaper article was shown me which was concocted by a sectarian practitioner formerly in New York, who detailed the inferiority of American medicine, schools, and practice to the horrified sanctity of the German public; and in the very week preceding the Congress, hundreds, or perhaps thousands, of pamphlets were distributed in Berlin for the avowed purpose of insulting us and making us uncomfortable. The pseudonymous author, who appears to have lived, or lives, in Chicago, says, among a great many other things, the following:

"In reference to the transatlantic gentlemen, nothing is more out of place than indulgence. American tolerance, so frequently extolled, exists for Americans only. When about to travel, they leave it at home. It is almost always the result of ignorance, indifference, and bad conscience. As the average American never cares for the history of a science, the majority of the transatlantic members of the International Congress are totally unacquainted with European institutions, labors, and scientific methods and their aims. Nevertheless, every one of these gentlemen carries a paper in his pocket, easily compiled, wherewith to resuscitate the obsolete science of Europe."

In the same sheet the man asserts that forty-two per cent. of all the doctors in Chicago are professed abortionists, and a great many followers of "Christian Science."

Some of the great Germans, with whose names every one of us is perfectly familiar, denied being in any way influenced by such rubbish; but then, again, it was through them that I was informed of a New York specialist, and

a fellow of this Academy, who was reported to have availed himself of his personal intimacy with the officers of the Associated Press for the purpose of having his Congress paper served at the breakfast tables of a million of American households on the day of its delivery. That was a week before the opening.

Thus you see, Mr. President, American medical gentlemen may meet with difficulties in the face of such occurrences. Still, though they are as human on the other side of the Atlantic as we on this, the facilities of communication between the continents have become such as to enable those wishing to see and know the truth that the time when American medicine was merely receptive and imitative has long passed by, and that we have entered the arena as co-operating peers. They were, indeed, anxious to have us and secure a large American attendance. In order to accomplish that end the organizing committee appointed an American committee, which was to enlist universal sympathy in our country. No time was to be lost, and the first ten medical men who expressed their willingness to serve were appointed. The territorial jealousy, one of the most marked American littlenesses, which found its way into print several times, has obliged me to explain publicly, in the May meeting of the Association of American Physicians, why that committee consisted of Stewart, Fitz, Lusk, Draper, Hun, Pepper, Busey, Osler, and Peyre Porcher. Will the Western gentlemen who found fault with the committee, and heaped vituperation on the mode of its composition, tell us that the names selected did not deserve the honor conferred upon them, or that there are better ones among us? Does American medicine begin at the Alleghanies or the Sierra? Or will you, gentlemen of Ohio, Mississippi, or Nevada, tell us which of the forty-four stars of the glorious flag is the one you claim as yours? Yours are the forty-four, so they are ours. Are your minds not big enough, your hearts not large enough,

to embrace the love of, and the pride in, the whole flag of America?

A further proof of the anxiety to secure the co-operation and good-will of the Americans was given by the Berlin committee in this, that they insisted upon one of the public addresses in the general meetings being delivered by an American. Weir Mitchell having declined in time and courteously, and Osler not being within reach, I was telegraphically directed to select an orator. The choice of Horatio C. Wood was heartily approved of in Berlin and elsewhere. Again a few have asked why a New-Yorker could not have been honored with that commission. That question is answered by some other queries: Do you know of a better man? Is America bounded by the East and North Rivers? And lastly, has New York forgotten that she can afford to be courteous and generous?

More, a few weeks only before the meeting of the Congress the American orthopædists expressed the desire that there should be a separate section of orthopædics. When I, then already in Europe, was notified of that request by the chairman of the Orthopædic Section of this Academy, and expressed my fear lest it might be too late to make arrangements for that change, I was by returning mail informed by the secretary-general that the request was at once granted by the Committee of Organization, on the ground that my countrymen must know best what suited them and their scientific labors.

Again, the organization of the Congress was not completed without the election of an American vice-president, John S. Billings, and an American, M. Allen Starr, as one of the two English-speaking secretaries, and a large number of American vice-presidents of sections. And lastly, when, on the third day of Congress and in the second general meeting, the hour grew late and the audience melted under the hot sun, Dr. Wood's address was, out of consideration for the Americans, postponed to be the first topic of the third meeting, though the hour

and arrangements and printed preparations had to be changed accordingly.

All this was meant, and was believed to suffice, to make every American feel at home. If it did not succeed, it ought to have accomplished that end. But I have been told that disappointments have been keenly felt and complaints been uttered.

When an English paper was read, many have been reported to have left the room. Many essays were not read at all, some were not allowed the time required by the authors, some men would read beyond the legal limits. Such comments are natural, but also their causes. The unprecedented number of papers offered at a late date, and too courteously accepted, and some acoustic disadvantages of many of the audience halls, are among the causes of disappointments, which are unavoidable in everything human. The experience of the past can furnish remedies in the future. However, when one man complains that he was not one among the five per cent. of members who could be admitted to the court reception in Potsdam; another, that he had to pay for his share of the section dinner on the evening of Wednesday, the 6th, proclaiming that matters were different in Washington, where no foreigner paid anything—it proves one of two things, either that there were those who went more for the incidental appurtenances of the Congress than the Congress, or that our national failing, which is a highly developed emotional hyperæsthesia, was rather demonstrative. I can assure those who are finding fault with the scantiness of their enjoyments that I know of one, at least, who neither shared in the entertainment in the City Hall, for which Berlin paid 80,000 marks, nor danced at any of the five balls, nor imbibed the music and songs in eleven languages, and as many beverages, at Kroll's, and —did not feel the worse for it the following mornings. If I have any fault to find, it is with the overflow of entertainments, the excess of generosity, the multiplicity of luncheons, dinners, and receptions, the waste of money

in the vast number of public and private social gatherings.

If there ever were hosts spending unstintingly—ay, squandering—money in the service of unlimited hospitality, they were the profession as a whole, and the single medical men of Berlin.

In connection with this fact let me make a remark, which is dictated by no cavilling spirit, that I have too many reasons to appreciate the universal kindness and untiring hospitality of the great and gentlemanly members of the Berlin profession, who were bent on nothing so much as rendering the sojourn of the foreign guests comfortable and pleasant. I must here mention the names of Virchow, Bergmann, Waldeyer, Gerhardt, Henoch, Martin, and Leyden and his accomplished wife, the chairman of the Ladies' Committee, and could name a host of others. Many of us have found it impossible to respond at the same time to the requirements of actual congressional duties and the urgent demands of hospitable courtesy. In this, also, there is discomfort and loss for the individual member. But the matter has a very much more important aspect. An excess of social entertainments on one hand, and the accomplishment of the end for which the International Congress is convened on the other, are incompatible at a certain point. Too many feasts interfere with legitimate work. The expectation of a good time may—if I must not say it does—invite the attendance of many, of hundreds, perhaps of thousands, who would not go for the sake of work. On the other hand, those who have gone for the latter are liable to feel sorely disconcerted. Thus it has happened—at least this disappointment can be held in part responsible—that the national associations have suffered from the persistent absence of those who do not wish to lose great opportunities; and that all over America, Great Britain, France, Germany, and other countries there have been formed by dissatisfied men, who place scientific work over any distractions, be they ever so pleasant, special

societies, the objects of all of which ought to have been accomplished in the sections of the general bodies. It would be a sad development if the same tendency were o grow up in international congresses. This very moment there are already in existence an international ophthalmological and an otological congress. It would be the fault of the management of international medical congresses if other specialties, or doctrines, should follow the example, for no other reason than the predominance of the social over the scientific element. If the latter cease to rule, the great men of science will stay away, and the holiday seekers and a few ambitious office holders will remain. *Experientia docet.*

It is only a wealthy city and rich professional men who can entertain as Berlin did. For such hospitality as was displayed there you require large and generous hearts. ample and well-filled purses. There are but few communities like her. If the habit of prodigality becomes persistent we shall be received in future with misgivings on the part of our hosts, who must fear lest their efforts fall short both of the results of predecessors and the expectations of the guests. Let these two calamities occur—viz., the absence of the best men of all nations, and, on the part of cities and men, hesitation to request our coming—what will become of the International Congresses?

And where is the prevention of the danger alluded to? Here: Let the social entertainments be reduced to a minimum. Then any city with ample hotel accommodations will be able to receive us, though we be thousands. Then those bent upon pleasure only will seek it elsewhere. Then the number will no longer be unwieldly and shapeless. Then the men looking for work, and for the men who work, will be eager to come and see and be seen, to teach and to be taught.

The unprecedented success of the American Congress of Physicians and Surgeons, the first meeting of which was held in Washington, in September, 1888, tells its own tale and exhibits the proof of what I say. In my mind

there is no doubt that its second meeting, in September, 1891, will be equally successful; its three days will be dedicated to work, and the official social entertainment limited to a plain subscription banquet. In that way neither the lawful work of the Congress nor private intercourse and hospitality are interfered with.

It may appear invidious to mention the co-operative services rendered by the members of the different nations represented in the various sections of the Congress. Still, as we generally have a good opinion of ourselves, we are not afraid of looking back at our own contributions to the scientific material that was furnished. When we do so we have to admit, however, that but a small percentage of our seven hundred participated in the general work. It is true there was one who got himself delivered of quintuplets; fortunately, he had no equals, and he was not, as a medical journal reported, "taken in earnest." Still, there were a number of papers, not compiled, but original. The Orthopædic Section was American to a great extent. The Neurological had a very fair representation from our country. The Gynæcological and Pediatric Sections were not without American contributions. The Surgical was supplied with papers which were highly appreciated, mostly from the West. Indeed, there were but few sections in which no American took part, though there were some in which no active work at all was furnished by us. The most redeeming feature was the meeting of the combined Laryngological and Pediatric Sections, in which the ingenuous, painstaking, and successful efforts of O'Dwyer were heartily applauded.

After all, however, the labor performed in the general sessions may be the principal, but is certainly not the only, object in view. An English journal has said that "congresses are not instruments of research"; and still the transactions of all are replete with it. It is true, a congress is not so much meant for new discoveries as for the broad dissemination of facts, hints, and ideas. A man —not being ubiquitous—may not take away with him

many things new, but what he carries home is a new stimulus and encouragement.

In the Congress you saw a great many men whom you thought you knew, but since you listened to them, and watched them while you listened, and took their measure, you know better now. You saw and heard the living objects of your admiration, the moulders of professional thought in all countries; discoverers, teachers, laboratory workers, practitioners; those who, after hard work, create books by spontaneous generation out of their brains, and those who compile them out of their pigeon-holes; the eagles, the bees, and the moles,—also the parrots, and that class of envious cuckoos who transfer other birds' eggs into their own nests. You found there is room in our great army for many men and many classes of men. You gathered encouragement from learning that even truly great men are still men and human; and that some degree of greatness is within the grasp of any man, in town or village, who will work for it intelligently, bravely, and honorably. All this is what a congress will teach those who consent to learn.

There is another lesson that is taught by a congress: The separation into twenty sections proves the endless and diversified branching of the grand old tree of medical science. Their working under the same roof, however, and under the same administration; their occasional combination for a common purpose; their gathering in general meetings, and their listening to the same addresses, with the same interest and profit—all this, in spite of the fact that some of the twenty appear to be threatened with the danger of degenerating into mere handicraft, proclaims louder than steeple bells that the medical science is "one and indivisible, now and forever."

The Congress has conveyed to me, like its predecessors in Copenhagen and London, a great lesson, and furnished an elevating spectacle. Imagine, those of you who have not been present, thousands of medical men from all parts of the world, and speaking a dozen different lan-

guages, not perhaps endowed with the same erudition or mental or moral power, but moved by the same instincts and interests, and assembling at the same call and for the same special purpose. The great and the lowly, the old and young, meet as brethren on the same platform, if not of equality, still of fraternity and solidarity. National jealousy and prejudice are shelved for at least a week, and the lesson is taught that brethren may live together peaceably under the same roof, an example to the nations both of the present and the future. The man and the man of science are appreciated and loved, though political adversaries. Applause takes the place of hisses. The contest is no longer against each other, but with each other, side by side, arm in arm, with the same weapons of the brain and soul against the common enemy of science and mankind, viz., physical deterioration and social misery. Thus the cosmopolitan spirit of coming centuries is foreshadowed and initiated by the co-operation of the men arrayed in the army of the noblest of all sciences and professions. Therefore, may no man who can prove an example to his peers in this or any other country, no man who can teach, none who can learn, none who can worthily represent his country in any capacity and do honor to America among foreigners—may no man, except for valid reasons, ever shirk his duty to attend an International Medical Congress.

ANNIVERSARY MEETING,

NOVEMBER 20TH, 1890.

FORMAL OPENING OF THE NEW BUILDING

OF

The New York Academy of Medicine.

COMMITTEE OF ARRANGEMENTS.

M. Allen Starr, M.D., *Chairman.*

Frederick A. Castle, M.D.
Arthur M. Jacobus, M.D.
Laurence Johnson, M.D.
Wm. H. Katzenbach, M.D.
Alfred L. Loomis, M.D.

THE HONORARY RECEPTION COMMITTEE.

Fordyce Barker, M.D.
Francis Delafield, M.D.
William H. Draper, M.D.
Everett Herrick, M.D.
Samuel T. Hubbard, M.D.
Abraham Jacobi, M.D.
Alfred L. Loomis, M.D.
William T. Lusk, M.D.
Charles McBurney, M.D.
Henry D. Noyes, M.D.
George A. Peters, M.D.
William M. Polk, M.D.
D. B. St. John Roosa, M.D.
Alexander J. C. Skene, M.D.
T. Gaillard Thomas, M.D.

THE RECEPTION COMMITTEE.

Robert Abbe, M.D.
L. Bolton Bangs, M.D.
Thomas H. Burchard, M.D.
Timothy M. Cheeseman, M.D.
Henry C. Coe, M.D.
D. Bryson Delavan, M.D.
Edward D. Fisher, M.D.
George H. Fox, M.D.
Thomas R. French, M.D.
L. Emmett Holt, M.D.
Frank W. Jackson, M.D.
George T. Jackson, M.D.
Walter B. James, M.D.
Richard Kalish, M.D.
Ruel B. Kimball, M.D.
Willard Parker, M.D.
A. Alexander Smith, M.D.
Richard Van Santvoord, M.D.

ADDRESSES MADE BEFORE THE NEW YORK ACADEMY OF MEDICINE,

At the Formal Opening of its New Building.

THE ADDRESS OF THE PRESIDENT,

ALFRED L. LOOMIS, M.D.

Fellows of the New York Academy of Medicine, Ladies and Gentlemen: I count myself most fortunate that it is my privilege to speak to you words of welcome in our new home. The only language which my heart prompts me to utter to-night is the language of congratulation—congratulations for the past, the present, and the future of our Academy.

Forty-four years ago representative men in different departments of medicine, actuated by a spirit of devotion to a high scientific purpose, founded this Academy. Interwoven with its early history are the names of John Stearns and John N. Francis, Alexander Stevens and Alexander Hosack, John W. Draper and Joseph M. Smith, Valentine Mott and Francis Delafield, and a long list of others whose names and medical achievements made honorable the medical profession of their day.

With such founders the membership of the Academy soon included most of the active medical workers in the city, and became the strongest and one of the most influential medical organizations in the State. From year to year it has grown in professional esteem and public confidence, and its advancing history has been stamped by the life and labors of such noble ones as Willard Parker, Alfred C. Post, James Anderson, Alonzo Clark, Edmond

R. Peaslee, Austin Flint, William H. Van Buren, James R. Wood, Cornelius R. Agnew, and a host of less prominent, but noble, self-sacrificing spirits around whose memories we delight to linger.

Time will not permit me at this hour to even mention the many important papers that have been read and discussed, and the large amount of original work which has become a part of the scientific history of this Academy. As one reviews the scientific work of those earlier years, he congratulates himself that he is a fellow of such a fellowship. An organization which has given so much to the profession as ours has a past for each one of us to be proud of, and I may well congratulate you this evening on the past of The New York Academy of Medicine. We step to-night into a present full of promise.

In my inaugural address, less than two years ago, I stated that in order that our Academy might become the centre of the scientific activities of the entire profession of this city, it must have a suitable home, a building that should furnish accommodations for a large and well-selected library, with reading rooms, and commodious meeting rooms for all our medical societies. Such a building is ours to-night, more elegant, commodious, and better suited to our wants and work than the most sanguine could have hoped for two years ago.

With a library capacity for two hundred thousand volumes, and a well-selected library of fifty thousand volumes, supplemented by the largest and best collection of journals to be found in this country, we may rightfully claim that we are in some degree meeting the highest requirements of scientific medicine. The influence which such a library will have, not only on the intellectual status and culture of the profession, but upon its moral tone, cannot be estimated, for there is an atmosphere about a large and well-selected library which does not favor the growth of a mean, money-calculating spirit; it conduces to broadness, tolerance, and a love of the higher and nobler attributes of man.

Our membership has reached nearly seven hundred, and includes most of the active workers in our profession in this city, and many in the State. Every specialty in medicine is represented by those who have become distinguished in their chosen lines of work. There are now established and well-organized sections in all the special departments of medicine and surgery, so that each fellow may find a place with congenial workers for making public the results of his own observations and experiments, under the sifting criticism of experts, and thus learn what may have been done by others in the way of support or in opposition to his own work. The combined scientific labor of our general meetings and sections is to a large degree leading and guiding the medical thought and research not only of our own country, but its influence is being felt in the medical councils of Europe.

We are also exerting an increasing influence on public thought and action. We are becoming a power in this city and State which is being more and more felt in the legislative and economic work of our commonwealth. The public health and safety of our citizens is being more and more committed to our hands, with the conviction that, by wise counsels and practical methods, we shall protect it from the ravages of disease by an ever-broadening and more perfect sanitary science.

Our fellow-citizens, in response to our appeals made to them during the past two years, have shown by their sympathy and liberal donations that they are recognizing more and more the importance of our work and its influence upon the general weal. Let us act wisely, energetically, and unitedly, and we may be assured that in the near future we shall be able to turn more largely the influence of the accumulated wealth of this great metropolis into channels for the support and encouragement of scientific medicine. Our profession was never so full of promise as at the present—never before were there so many strong men in its front ranks as now, never were

there so many cultured and brilliant minds entering it as to-day. If this great and daily increasing power can be centralized, as is possible, within these walls, its influence on the social, domestic, business, and religious life of our city cannot be estimated; already the better minds in all departments of science are turning to us for help and inspiration. There is no longer a strife of sects or creeds, but a struggle for the supremacy of intellectual power and broad culture over weakness and charlatanism. There is no place in the broad field of scientific medical inquiry for the would-be medical man who talks of the potential power of infinitesimal abstractions and the so-called scholastic illusions. We are living in, and are part of, an age of facts, not fancies; work, not theories. This epoch in medicine is indeed more brilliant and eventful than any which has preceded it, and the prospectus grows constantly more encouraging and richer in possibilities. The effect of the many enthusiastic workers who to-day are struggling for truth in medicine must produce a general advance, notwithstanding, as Dr. John W. Draper once said, "there has been through all the ages, constantly hovering about honest workers in our science, a host of impostors and quacks, who will continue to thrive so long as there are weak-minded and shallow men to be deluded and vain and silly women to believe."

I congratulate you then, that, with the most advanced workers in every department of scientific medicine gathered in our fellowship, we meet to-night about our own hearthstone, in full possession of all those faculties which are needed to render our work most efficient and stimulate us to still higher achievement in the future, so that the work done in these halls shall have a forming and crystallizing power on the medical literature of our whole country.

The days of doubt and anxiety are past, success has ceased to be a question, the auspicious present marks the beginning of a new and broader career for our Academy. It is here, in the centre of this great city, to do its part in

stimulating its intellectual and moral forces. With increasing opportunities come deeper obligations. Our future must not be gauged by past successes or present advances. We are under obligations to the past, but under bonds to the future. As we in turn pass this trust to our successors, to those who in the future are to be the exponents of the lofty mission to which this building is dedicated this evening, it must not have suffered in our hands, but have grown and broadened under the impulse of true enthusiasm and faithful work. Yesterday we read the history of the past, to-day we make history for the future ; and whether he will or no, every fellow in our number must leave his mark, be it little or much, in the records of this Academy. Let us be inspired, then, with the thought that our fellowship shall become a power in raising our profession to the high place which the future shall assign to it, centralizing its influence, and elevating it socially and intellectually to a position which was not hoped for in the past or attained to in the present.

Our future must be, and will be, interwoven with the many and rapid transformations that are to take place in every department of science, and our relations must become more and more intimate with the great public, who are fast realizing that the first mission of our labors is the prevention of disease. The career of the physician of the future will be nobler and pleasanter, because he will have less of ignorance and prejudice to combat, but he will require a higher culture than his representative of to-day. Here in this library and in these halls both the medical profession and the philanthropist will find that inspiration which shall give birth to a greater devotion to the alleviation of human suffering, and a better realization of our duty in the elevation of the masses to a higher civilization. We must never look backward, but always forward. Provided with the machinery of wisdom we have inherited, but not wise in our own conceits, let us make this building the great workshop, where the fires of

scientific enthusiasm and persistent labor shall smelt from out the ores of our daily experience the pure metal of truth that, cast in the moulds of patient thought and polished by the sharp emery of keen and kindly criticism, may some day furnish to generations yet unborn armor and weapons with which they will advance victorious over all the forces of death—save threescore years and ten.

So shall the congratulations which we utter to-night re-echo from these walls when other voices recall this day. Join with me, then, in thanksgiving to the Great Physician for what has been accomplished, and in this invocation, that these walls may not crumble or cease to shelter faithful, earnest, Christian men until suffering humanity is free from its bondage to lust and excess, and is victorious in its struggle against the invisible arrows of disease.

THE NEW YORK ACADEMY OF MEDICINE.*

By E. L. KEYES, M.D.,

NEW YORK.

Mr. President, Fellows of the Academy, Gentlemen and Ladies: If there are sermons in stones, then an address has already been delivered to you to-night on entering these walls, in a strain of greater dignity than any to which I may aspire. The fact of the existence of this building at all in the graceful form in which you see it, devoted, as it is to be, to philanthropic and humanitarian ends—this in itself is a whole volume, a canticle of praise to the energy of its promoters, and a pean of gratitude to the liberality of those whose material bounty has made its erection a possibility.

Those of us who are to possess this well-equipped arena for scientific effort, this nucleus from which shall radiate ever-broadening lines of medical thought, have lived to see a good day, and to enter the promised land toward which our earnest expectancy has led us through many a long hour and weary year; and in contemplation of the work accomplished, in that grateful lassitude which accompanies the consummation of a successful effort, we might perhaps with better grace muffle our ineffective voices and allow the stones to discourse to you in the eloquence of their majestic silence.

Yet this may not be. Some articulate words are called for, and if I, who have been honored by being made the mouthpiece of my fellow-academicians, can, as an im-

* Being an address delivered on the occasion of the opening of the new building, November 20th, 1890.

personality, render to you for them any words in harmony with the occasion, the accomplishment of the pleasant duty will be its fitting reward.

An academy is an institution sanctioned by illustrious precedent in that group of devoted followers who, in the groves of the suburb of Athens bearing this name, in the year 348 before Christ, clustered around Plato as he "taught the truth"; and to teach the truth after investigation has been the proper function of the academy ever since that day. The various institutions of learning which in different scientific fields have borne this name have made for themselves everlasting renown, and have established a criterion by which, if our little body of earnest workers is to be judged, it behooves us to spare no effort that our results may be deemed worthy to be enrolled upon the same scroll of honor.

The age in which we live is distinguished notably along three prominent lines: by material progress, by the broad dispensation of rational charity, and by the far-reaching effectiveness of scientific study in its practical application to the needs of mankind.

This building in which we are assembled represents a crystallization of the essence of all these lines. The material progress is represented by the graceful outlines of the building and its commodious internal structure, which the architect has ably conceived and the workmen faithfully executed. The very existence of the building is the acme of a broad charity, since it stands for the accumulation of many hard-earned dollars; that this institution may live and become effective, not solely for those who make immediate use of it, but that the fruit of their labors may spread abroad through the land for the benefit alike of all who are in need; and it stands for science, for it is the rostrum from which science speaks; it is the arena in which science contends; it is the soil in which are implanted the roots of that tree of medical knowledge, in the branches of which the investigator may find the bud, the flower, the ripening fruit of past experience; it is the

fountain from which shall emanate rivers of refreshing sweetness to cool and succor the parched sufferer along the dusty highways of disease.

And what to say of the academic body itself, of which this edifice is the outward and visible sign? Conceived, as its historians have often narrated—conceived in the spirit of good-fellowship and brotherly kindness on the evening of November 18th, 1846, at a dinner of the Society for the Relief of Widows and Orphans of Medical Men, it took shape December 12th, 1846, with the sanction of about two hundred and fifty physicians, under the immediate direction of Alexander H. Stevens, assisted by Parker, Watson, Mott, Isaac Wood, Smith, and others; was born on January 6th, 1847; and baptized by legislative enactment of incorporation June 23d, 1851.

The motive for its formation was stated by its founders to be the recognition of a "lack of harmony and concentration of effort for scientific purposes in the profession," and a desire to elevate a barrier against quackery, which at that time, it appears, had reared a more formidable front than before or, possibly, since that day. At its birth its future functions were defined to be: 1, the cultivation of the science of medicine; 2, the advancement of the character and honor of the profession; 3, the elevation of the standard of medical education; 4, the promotion of the public health.

The Academy is therefore now in its fifth decade. It has lived through its babyhood and period of riotous youth, homeless at first, wandering about seeking shelter, and having no roof to call its own, until well along in its twenty-seventh year, when on December 24th, 1874, it secured, largely through the energy of Dr. S. S. Purple, a permanent abiding place at 12 West Thirty-first Street.

There might almost be said of the Academy what has been tersely written of the life of man, dividing it into decades:

"At ten, a child,
At twenty, wild,
At thirty, sound—if ever—
At forty, wise,
At fifty, rich,
At sixty, good—or never,"

except that the Academy has been from the beginning good—a quality which may not be affirmed with equal confidence of all men.

For surely at ten the Academy was a child, and a wanderer in the streets; at twenty we may be pardoned for declining to inquire into her follies; at thirty she was certainly sound—indeed, there never has been a question of the health of the organization; at forty who shall deny that she was wise? for it was in the early forties that the necessity for expansion was felt, and that spirit generated which has culminated, in this our forty-fourth year, in the completion of this modern home, in which the treasures of our library will be adequately protected from fire, and wherein ample provision has been made for the convenience of present work and future expansion. And in signalling this triumph of the Academy's fifth decade, it is impossible not to pause and pay tribute to our President, Dr. Loomis, whose energy, zeal, and ability have contributed so largely towards the accomplishment of the result.

It is hardly necessary to carry the simile further. At fifty the Academy cannot fail to be rich. She is rich now in the love of her children, in the respect in which she is held by the community at large; she is rich in her library, and in the accumulation of good work by her members. This is the material wealth of a scientific body, and of this she has already a fund and a steadily increasing store.

At sixty the Academy becomes immortal, and will remain until the consummation of time, a pillar of beauty and strength, an integral part in the grand temple of science which is being reared by zealous and loving hands throughout the length and breadth of the entire earth.

And how has the Academy fulfilled the aspirations of

her founders? Surely the end is not yet, and more remains to do, but in the four directions which were defined at her origin as her special lines of effort her advance has certainly been satisfactory. The science of medicine has been cultivated; the division of labor into section work has brought together spirits scientifically akin, and the quality of the material presented to, and digested by, these sections is of a high order and of steadily increasing excellence. The character and honor of the profession have been sustained; struggling factions have been dominated by wise counsels, and threatened rupture averted by the exhibition of a broad spirit of professional charity which has helped to steady and elevate the quality of the professional gentleman, without as well as within the academic circle.

The weight of the Academy has always been thrown into the scale to help to raise the standard of medical education, both by the personal effort of the fellows, many of whom have occupied high positions as instructors in the various institutions of learning, and in efforts to help shape legislation toward the accomplishment of the same result upon a large scale.

That the promotion of the public health has been an object of academic solicitude is witnessed by the present existence of our efficient City Board of Health, which was conceived and formed in the bosom of this Academy in the interest of the citizens of New York.

The workers in the academic field have not been very numerous. The present roll, the largest ever possessed, numbers seven hundred; but in that number may be found the name of nearly every living physician of recognized eminence in the city and immediate neighborhood; and it is difficult to mention any of the illustrious dead of our profession whose names will not be found written upon the roll of the Academy, as well as imprinted upon the memory and in the hearts of those who loved them for their kindliness during life. How shall I mention any without slighting more? Francis, Mott, Stevens, Parker

with his genial smile; Watson, Post, Peaslee, Flint, the good physician, the crystallization of benevolence; Rogers, Bumstead, Buck, Wood, the man of action; Delafield, Clark, Dalton, Anderson, Agnew, the Christian gentleman; Hamilton, Sims, Van Buren, the man of judgment, the man of dignity, a very man, a prince among his peers; but why prolong the list? The good men have been ours, the good men are ours, and their work, the best of it, is fostered by this Academy and turned to good account.

And yet the high success of the few is not the measure of the usefulness of the Academy. These illustrious ones would have glittered without the Academy. Their lustre is shed back and illumines the whole body in which they mingled, and multiplies there for the good of the community at large.

We, in our scientific struggle and effort, are much like children. The vastness of the field belittles our personality. The pretentious few who arrogate to themselves a personal superiority are more than liable to be left behind by the patient seeker after truth whose path is lightened by the glowing rays of human kindness. Children we are indeed!

"We go forth like children in the morning,
Scattering, to spend the summer hours;
One his brow with laurel wreaths adorning,
One to saunter 'mid a grove of flowers;

"One to lose his way, and wander, straying,
Till the twilight, frightened and alone;
One, it may be, wearied with his playing,
Wending home his footsteps e'er the noon.

"But, whatever fate to us is given,
All, when day is done, again shall meet,
And at nightfall, 'neath the stars of heaven,
Shall be gathered at our Father's feet."

To obtain an idea of the relative standing of this Academy, we must compare it with other analogous institutions. The Imperial Academy of Science in Vienna, and that of France, are scientific, not medical, bodies. There

is a Royal Academy of Medicine in Belgium, one in Italy, one in Ireland; there is an American Academy of Medicine, one in Kansas, and one in Detroit, but the moderate scope and importance of these various academies relieve them from comparison. Germany is justly proud of her two distinguished associations in Berlin—the Medical Society under the presidency of Virchow, and the Society for Internal Medicine under the leadership of Leyden. Illustrious names glitter in each of these constellations, but neither has a building of its own yet, although the Medical Society is now erecting one in company with the Berlin Surgical Society. The library of the Medical Society is about two-thirds the size of our library, while that of the Society for Internal Medicine is insignificant. The great Austrian medical body, the Royal Imperial Society of Physicians, in Vienna, justly renowned for the brilliancy of its work and the standing of its members, has no building of its own, and a library of something over eleven thousand volumes, not one-third the size of ours, although the society is ten years older. Of the Surgical Society of Paris, and other foreign medical associations, it may be said that, whatever their distinction, they are not sufficiently analogous bodies to be fairly compared with our Academy.

Two foreign institutions, however, fulfil the conditions: The Academy of Medicine in Paris, and the Royal Medical and Chirurgical Society of London, upon the general plan of which our organization was outlined.

As between these two and ourselves, at this date, in evidences of material prosperity at least, our Academy holds its own. As to the scientific standing of the members, I shall not draw comparisons. It is enough to say that each of them contains the flower of the medical science and art in the districts in which they are respectively situated. In this country there is no other Academy (except the relatively unimportant ones I have mentioned), although there are many notable Medical and Surgical Societies; but the extent of our land, and the

widespread distribution of its talent, make it probable that others will shortly arise.

Comparing, then, some of the main points in the three that I have selected, I may say that each has a building of its own—that of the French Academy being a temporary one. They have the funds, and propose constructing a suitable home in the near future.

The Royal Medical and Chirurgical Society, London, founded 1805, membership seven hundred, seating capacity of largest hall three hundred, area of hall forty by fifty feet, foundation area of building fifty by two hundred feet.

The Academy of Medicine, Paris, founded 1820, membership one hundred and ten, seating capacity of largest hall ninety-two, area of hall small, foundation area of building small.

The Academy of Medicine, New York, founded 1847, membership seven hundred, seating capacity of largest hall three hundred and fifty, plus extra opened-up space two hundred and fifty, equals six hundred; area of hall forty-two by fifty-seven feet, and two extra smaller rooms that may be opened into it; foundation area of building seventy-five by one hundred feet.

All have libraries, but on this point we may seek a wider field for comparison. Our library, which is, I believe, the youngest on the list, and which always gratefully recalls the names of its chief munificent donors, Purple, Dubois, Bumstead, Stone, Jacoby, and many others whom time forbids me to detail, was founded by donations in 1877, never bought a book until 1879, eleven years ago, yet now, safely housed in a fire-proof home, we are proud in possessing the third place numerically among the medical libraries of America, and the fourth place, as far as I can learn, among the purely medical libraries of the world. In this country the library of the Surgeon-General's office in Washington, founded in 1865, is the largest, that of the College of Physicians in Philadelphia, more than one hundred years old, the second.

Comparative Table of Medical Libraries.

Name of Library.	Date.	Volumes.	Journals.	Current Journals.	Pamphlets.
Academy of Medicine, Paris	1820	130,000	18,000	390	
Surgeon-General's Office, Washington	1865	97,886	33,173	over 700	144,887
College of Physicians, Philadelphia	1789	45,000	.....	400	
Academy of Medicine, New York	1847	40,000	.....	400	
Medical and Chirurgical Society, London	1805	36,000	.....	150	
Medical Society, Berlin	1839	about 30,000			
Royal College of Surgeons, Dublin	....	about 25,000			
Medical Library Association, Boston	....	19.365	.....	381	19,100
New York Hospital Library, New York	....	18,386	.....	109	No record kept.
Royal Imperial Society of Physicians, Vienna	...	11,069	.....	132	
Aberdeen Medical Society, Scotland	....	6,000			

So stands our Academy, and such she is when compared with other analogous institutions in other parts of the world. Considering her age, she need not be ashamed. To fulfil her destiny and consummate her function requires only a continuance of the zeal which has attended her development from the first, and a common impulse among her members to work for work's sake.

Here in this hall, now radiant with the gracious smiles of approving friends, must be fought out many a desperate scientific battle. Clad in the armor of scientific method and wielding the sword of personal experiment and investigation, contending individuals and contending factions shall battle for the supremacy of their ideas, until these walls shall resound with the din of conflict; and from the blows given and taken with such weapons upon such honest armor there shall scintillate and radiate sparks and flashes of truth, living fire, to be added to, and heaped upon, the burning flame that glows forever upon that common altar of Science at which we all worship, a flame to act as a beacon of safety upon the hilltop to en-

courage those for whom the battle is fought—the patient, suffering victims of disease—and a flame which, within this academic body, shall serve as a cloud of smoke by day and a pillar of fire by night to guide and guard the honest investigator in his never-ending endeavor to teach the truth.

OUR LIBRARY.*

By A. JACOBI, M.D.,
NEW YORK.

Mr. President, Ladies and Gentlemen: A circular, published by special committee of this Academy in January, 1888, contained the statement that the New York Academy of Medicine was an incorporated institution, then more than forty years old; that its object was the cultivation of medical science and art; and that this aim was, among other means, reached by maintaining reading rooms which furnished nearly all the medical journals of the world, and by collecting a library which was—and is to-day—free to the fellows of the Academy, to the whole medical profession indiscriminately, and to the public at large. Our library was steadily increasing, the capacity of its shelves strained to the utmost, the building not fire-proof, and our accumulated treasures were in constant danger. For these reasons we appealed to both the profession and the public for aid in procuring for our meetings and our books a fire-proof building large enough to accommodate two hundred and fifty thousand volumes, spacious enough to afford quarters to all the scientific societies of the city, stately enough to worthily represent the medical profession of the metropolis, and able to testify both to the unity and earnestness of that profession and the sympathy of the city, which at the same time is the largest in size and the greatest commercial power of the continent.

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* Being an address delivered on the occasion of the opening of the new building, November 20th, 1890.

This library of The Academy of Medicine had a slow but steady growth. Thirty-three years ago, when I was admitted to membership in the presence of the great and good men who then were the guiding stars of the profession, Alexander Stevens, Valentine Mott, Horace Green, Gurdon Buck, Edward Peaslee, Edward Delafield, John Francis, John Watson, Ernst Krackowizer, there was no library at all, not even a medical reading room, in the city. It took many years before the Journal Association was organized, which furnished, in a room fitted up for the purpose in 64 Madison avenue, the current medical journals. Other years elapsed, until an amalgamation of the Journal Association and The Academy of Medicine, then in 12 West Thirty-first street, was brought about. The accumulation of the annual volumes, and a valuable collection of American journals and other books presented by two fellows, were the first stock of the library. The journals were paid for by an appropriation of the Academy, which, being small in the beginning, for many years amounted to from three to four thousand dollars annually. More could not be spared. Thus it was that we could not purchase new books. Occasionally a sum was raised by voluntary contributions for the purpose of buying the collection of a deceased member, certain publishers would present us with their publications, authors donate copies of their writings, fellows and others give old and new books, and men interested in special branches of literature furnish a shelfful of special works. The largest and most valuable addition of the kind was bequeathed to us by the great specialist, Dr. Freeman J. Bumstead. That was our library. Thus it grew slowly but steadily. In the course of years our stock of journals became more and more valuable, but what we wanted was a regular supply of new books, for which we had no funds at all.

On October 2d, 1889, when I had the honor of addressing you at the laying of the corner-stone of this edifice, I could refer to the fact that at last we had, for the pur-

chase of new books, a special library fund of ten thousand dollars, half of which was a memorial gift. For the same purpose and in the same spirit the widow of a deceased fellow and vice-president has since presented another special fund of ten thousand dollars, so that one-fifth of the sum required for the perpetual endowment of the library is now secured. We are thus approaching the time when New York City will possess a medical library fully adapted to meet its ends. What are they? A large library, besides being the proof of existing culture and accumulated intellectual labor, fulfils its destiny by giving information. Here the medical man with scanty means will find his text books and monographs to aid him in unravelling the obscurities of a difficult case on hand. He with an ample library of his own will come here to consult rare books, old journals, expensive works. Here all the journals of the world may be consulted from day to day; here those who are engaged in literary pursuits find their historical records. But what a library is most successful in is the inculcation in a great many of the habits of study and research. In that result the public is very much interested. Its safety and dignity require cultured and erudite physicians.

In the same degree that the ethical and intellectual standard of society is raised, the community will demand a higher standard of education and culture on the part of its liberal professions, among them the medical. A profession is called liberal in this, that it is generous, charitable, and high-minded; in this, that it liberates its members from ignorance and mental and moral hebetude. But in reality the medical profession of the country was mostly liberal in this, that it admitted to its ranks uneducated persons of all colors, sexes, ages, and previous conditions of servitude and illiteracy. Instead of being a truly liberal profession, it has merely been too liberal. In this tendency it has been encouraged, or rather this inferior standard has been forced upon the medical profession, by the public. He who requires manners in his corn-

cutter, and demands gentleness in his tailor, would often not object to selecting for his family physician and public hygienist a medical adviser with the orthography of a village school, the touch of the corner grocer, and the mental level of a soap peddler.

From this depth the profession has risen spontaneously by study and its indigenous moral development. Not all of you know, however, to what extent you are under obligation to the medical profession. Fifteen years of incessant agitation were required to finally pass the bill for the establishment of a State Board of Medical Examiners. If in future you will be protected against practitioners who have nothing to show besides their diploma, granted by a college—no matter of high or low standing—if the license to practise on you, your parents and children, will be made dependent on a second examination, you owe that blessing to the exertions of the medical profession. You might have made the result more striking. If the public had understood its interest you would have worked with us in behalf of making the State Board one, and not three.

Another achievement of the profession which concerns you as much as it does us, is the final passing of the bill requiring some degree of general education on the part of every medical student who expects to obtain his medical diploma. Thus a step is made in the direction of rendering the profession more liberal, more cultured, more effective, more fit to take charge of the most sacred offices that can fall to the lot of men. For the holiest and greatest of the objects of human study and care is man. That is so much a part of the creed of the medical profession that you can imagine the painful and contemptuous surprise at our learning that a medical man, in a public position, but fortunately not one of us, worked all winter to have the law repealed.

Fortunately not one of us. For from its very first days this Academy of Medicine had the elevation of the standard of medical education and culture inscribed on its

banner. That object has become such a settled axiom in the mind of every fellow, that, years ago, it was no longer considered necessary to retain it in just as many words among the written laws. In this tendency you can sustain the efforts of the profession. Insist upon this, that your physician be a gentleman and a scientist, and do something for that purpose yourself. For the State does not contribute to that end. The State is only society organized for certain purposes of co-operation and protection. But medical education, though ever so indispensable for the pursuit of health and happiness, and the training of erudite and liberal physicians, has not been recognized among them. But you who do not say to the hungry, the cold, and the naked, "Be ye fed, be ye warmed, be ye clothed," without helping them to food, fire, or clothing, must not expect a profession that always works in the private and public interest of yourself and all those dear to you and yours to be at once learned, erudite, and wise, and refuse aid in its efforts to perfect itself and benefit the commonwealth—aid by pecuniary support, by your social influence, and also some occasional gentle political pressure on our representatives in Albany.

Our greatest drawback has long been that we had no large class of learned medical men, such as study for study's sake, irrespective of pecuniary gain. Our profession has always consisted of practitioners. The necessities of life have acted upon the medical fraternity as on the community at large, which knew but exceptionally of art, of music, of philosophical refinement, as long as the country was still wrestling with the difficulties of the soil, the insufficiency of commerce, and the hamperings of poverty. Thus the immense majority of the medical men of the country gloried in being practical, and that only. That there were architects who never laid a brick, mathematicians who never triangled a mountain, astronomers who never sailed a ship; that no cathedral, no coast survey, no ocean travel could exist without them; that indeed there is no rational practice without an underlying theory,

was not considered. The very strongholds of medicine, histology, physiology, the fields of experimental labor and microscopical research, all those branches which you cannot immediately exchange for cash, have been neglected among us until lately. Like special laboratories, it is but a short time since great medical libraries have sprung up in Washington, Philadelphia, Boston, and New York. The sooner we admit that we have been far behind Europe in that respect, the better for our scientific future. Indeed, the intellectual maturity of a nation can best be measured by the amount of its original and unpaid research. Europe knows that thoroughly. The intellectual atmosphere of Paris depends greatly on its universities. The universities of Germany, with their independent workers and thinkers, have always been the pride of the nation, even in the distress of national poverty and political humiliation. In all of them the principal means of information, through centuries, have been their large libraries. And it will be our library round which the scientific interests of the profession will largely centre; but not of the profession only, for the Academy, as it opens its doors to whosoever will attend, without regard to membership, has always held that in order to increase the number of its beneficiaries it must make its library free. This is so well understood and so highly appreciated that the city has released the taxes on its building. A medical library contains of necessity many works and journals of interest to professional men besides medical. The lawyer and physician have many studies in common. There are in the city two societies for the special study of forensic medicine and medical jurisprudence, both of which can be better studied in a medical than a legal collection. Nor is a medical library, such as we have and mean to increase, a forbidden fruit to the intelligent, well-informed non-professional man or woman. Fortunately, there are a great many good popular works, besides those compiled for an ephemeral mar-

ket, which treat of physiology, hygiene, statistics, and other topics of universal interest.

Therefore we hold that the profession has a right to look to the public for appreciation and aid. We are not situated as they are in Europe, where educational institutes, as they are controlled, are also supported by the government. For the democratic spirit of our social and political institutions is opposed to centralization of that kind, and the generosity of the citizens has often been appealed to, and hardly ever in vain. There was a time when the church, centralizing all information, beneficence, and social and political influence, was the only legatee of the rich and benevolent. Now there are a hundred opportunities for liberal outlay. To select the proper ones is an art. I suppose it is a great achievement, which only a few select ones can attain, to make money; but it is a greater art to spend it both generously and profitably in the interest of science and charity. The greatest of all charities, however, is to benefit mankind by levelling the road of science. It is not millions we want. A hundred thousand dollars will clear this temple of sciences from debt, and swell our library fund to a sufficient sum, the interest of which will forever supply us with everything medical and scientific that will appear in any country. Well-to-do ladies and gentlemen will, I hope, not leave this building without making up their minds to contribute their share to the extinction of a debt which the community owes to the profession and to itself through improved educational facilities. "Let your light so shine before men that they may see your good works."

In conclusion, my friends of the profession may permit an additional word or two on the subject of the library, which is so dear to all of us that it was selected as the subject of a special address to-night. In one of its retired nooks I was sitting a few days ago, contemplating its past and future. I sat wondering how long it will take, and whether any of us older men will see the day, when America, after having given the political world the

guiding example of a stationary popular government both conservative and perfectible, will lead the world of science, as it does that of politics and, we hope, of healthful social development; wondering, also, how much this head centre of the medical profession and this ever-growing library will contribute to that consummation, which you can hasten by industrious, honorable, and modest work—but by work only.

This library of yours has started from small beginnings, like medicine itself. It comprehends the labors of thousands of workers assiduously employed through long centuries. That one of them could be missed it is difficult to say. For the co-operation of the many, the gradual development of ideas, the slow changes in experience and doctrines, are of as much importance as the revolutionary and epoch-making labors of the greatest. For no single man can stand alone, a law to himself and others. Even genius is the child of its time. No Washington or Lincoln, no Hippocrates or Aristotle, no Virchow or Pasteur, or even Koch, none of these immortal ones is a world by himself, and an isolated, self-lit sun, illuminating and warming the universe. Every one has been raised on the shoulders of his predecessors. By that knowledge it is that while hope and energy are aroused, patience is taught to the individual and the profession. For while life is short, science and art are unlimited and eternal. And the comparison of what you furnish yourself with the existing mass of accumulated knowledge, inculcates modesty and enhances zealousness. Thus good citizens are made and model scientists. Besides, what to the pupil the information gathered from the lips of his master, that is for you the collective bequests of all centuries as represented in your library. Thus an intellectual kinship is formed between you the living, and the spirit of all eras of history. That is what the study of the history of medicine teaches us which we have so long neglected.

Pondering over the shelves you behold abstract scientific treatises, works on practical therapeutics, and books

on art and appliances—all of them composing our beloved "medicine." Remove the theoretical works on anatomy, histology, and embryology, experimental physiology, physics, and chemistry—what remains? The wreck of the edifice, the foundation of which is torn away.

Look at the shelves holding special literature. There the specialist will comprehend that his doctrine and art are but a minimal trifle when compared with the surrounding wealth, and that the basis and link of all specialties is general medicine. Every one of them evolved from a minute bud of the great tree, and but few have ever been able to grow up with anything like independence. Thus medical science and art is shown to be an organism of slow, consistent, historical growth. Even the very excrescences—call them fallacies, superstitions, theories, schools, or sects—do not disturb the organic economy. In accordance with this, your very library, the representative and exponent of all medicine, is no longer a mere collection, but a vitalized organism.

That is why there is an atmosphere of solemnity in your large library; for you are standing in the presence of the spirit and soul of all previous ages, each evolving from and connected with its neighbor. That is why a library is to the scientist what the church is to the pious; or a museum of a hundred gems, like that which a generous fellow presented to our reception room, to the artist. No consideration of lucre invites you there. While nourishing your minds, you disconnect yourself from the embarrassments of trivial employment, and deliver yourselves from the merely terrestrial. In that way idealism is nurtured, that no feeling and thinking man is to be without; idealism, without which no nation can expect to live. When she lost it, even Hellas perished, though she had given birth to Solon, Pericles, Aristides, and Sophocles.

Let me suggest this reflection as a platform, my young colleagues. It is not a dream, but a reality, if you will make it so. By so doing, not only will you elevate your august science and the noblest of all callings, but you will

also remain in constant and indissoluble intellectual and moral contact with the most cultured elements of society. If you do, this evening, which is both an anniversary and an inauguration, will prove a blessing for all future to both the profession and the community. Look upon this edifice not merely as a new and commodious building, but as the visible portal into a new epoch. If you do so, you will consecrate this solemn occasion as the Fourth of July of American Medicine.

ADDRESS *

By FORDYCE BARKER, M.D.

Mr. President and Fellows of the Academy: I must detain you only a few moments on this joyous occasion, to express my warm congratulations. It is now nearly twelve years since I had the honor to first address officially the Academy, when I expressed the hope that the walls of our then home would soon be extended for our rapidly growing library. Within a few months after, by the spontaneous, munificent gift of one whose name must ever be gloriously perpetuated and now honors one of the rooms of the present building, our hall was greatly and beautifully enlarged, and met all our wants for several years, until it became apparent that we must have more room for our books and the other requirements of the Academy. To most of us this probably seemed a dim perspective in the future. Who could have anticipated so speedy and noble a result as we now see? We can now say appropriately, in a paraphrase of the words which Shakspere puts in the mouth of Gloster, in "Richard III.," "Now is the winter of our discontent made glorious summer" by these our sons of York, and, in grateful remembrance of many of our liberal benefactors, I must add, the daughters of York. I must congratulate the Academy on its office-bearers, all of whom have worked most zealously to bring about this happy result; and especially congratulate it on its wise election of the present incumbent of the presidential chair, and his immediate predecessor, both of whom have labored together with

* Delivered on the occasion of the opening of the new building, November 20th, 1890.

untiring earnestness, great wisdom and tact, and fortunately both of whom possess means which they have contributed to this end. I do not hesitate to express the opinion that without the combined efforts of these two gentlemen the glorious consummation which has now arrived would have been long delayed. We have now a central and spacious home in which the profession will find it pleasant and profitable to work together for mutual improvement and the public good, in the advancement of science, the promulgation of new truths, and the development of progressive skill in our art; and, I will add, the cultivation of those social graces which bind us in the friendly ties of a brotherhood, a noble and useful profession. We can assure the profession that they will be welcome to a library which contains the accumulated treasures of the past on every topic pertaining to medical science, and the current and periodical literature of the day, so necessary for all who would keep abreast with the present rapid advance of science. We to-night begin a new era of The New York Academy of Medicine. Who will venture to cast a horoscope of its grand future? New York, the commercial metropolis of the country, should be the metropolis of the medical thought, the medical literature, the medical teaching of the country, and to The Academy of Medicine belongs this great mission to make it so. One thing we should all remember, "that above," to quote from Webster, there is room for an additional hundred thousand volumes. Ever bearing in mind that "Concordia parvæ res crescunt; discordia maximæ dilabuntur," and the future we seek for this Academy is certain to be gained.

Mr. D. Willis James, who was to have delivered an address on "The Influence of Scientific Associations upon Great Cities," sent a letter regretting his inability to be present, and stating further: "New York cannot learn too soon the fact that a great imperial city cannot be built on material prosperity alone. The foundations must be deeper, broader, and more enduring. Years ago, in the city of Würzburg, I was deeply impressed by the fact that the magnificent palace begun centuries ago as a home for regal magnificence, luxury, and display, was going to decay and was shrouded in gloom, while a hospital started at the same time, to be of service to the needy and the suffering, was, after the lapse of the centuries, doing its beneficent and holy work, and out of which had sprung a medical college, the fame of its distinguished professors drawing crowds to the city.

"If New York is to be the great imperial city of the future, we must see to it now that we plant and foster not only churches and schools, hospitals, art galleries, and parks, but also great universities, largely endowed, and, as most important parts of these universities, medical colleges, magnificently and munificently endowed, so that they shall have every possible facility for accomplishing the best work. . . . Looking at the magnificent work the medical profession have done and are doing, remembering all they are accomplishing for suffering humanity, stirred as our enthusiasm is by the discoveries of such men as Professor Koch, of Berlin, with their promise of lasting benefit to humanity, let us, as citizens of New York, see to it that in our midst the means are amply and promptly supplied for the most complete scientific research in all departments of learning, but especially in medical science."

CONGRATULATIONS FROM WASHINGTON.*

By JOHN S. BILLINGS, M.D.,

U. S. A.

"I ESTEEM it a great privilege to be present on this occasion, and congratulate the fellows of the Academy most warmly and heartily upon their acquirement of a permanent home. I offer these congratulations not only as an individual warmly interested in the progress of the work of the Academy, but in behalf of the medical officers of the army, and of other departments of the Government, who of late years had experienced much hospitality and courtesy in the rooms of the Academy while stationed in New York City and its vicinity. While many things have been gathered together beneath this roof, for the benefit and use of the members, yet the central figure in the eyes of many, and certainly in my own eyes, is the library and reading rooms. In this hall one can meet with distinguished, learned, and skilful men of the profession living in this city, and obtain knowledge and advice from them upon subjects which they might treat of; further, in these rooms one can meet with the great and illustrious dead and the living of the profession of all countries, ready to give information upon all subjects regarding which one might choose to consult them. They were never obtrusive, they were never in a hurry to get away; they would wait one's time with infinite patience."

Basing some remarks upon personal experience in what he facetiously called a "branch" of this library in Washington, the speaker mentioned some things which he

* Abstract of an address delivered on the occasion of the opening of the new building, November 20th, 1890.

thought it most desirable a library of this kind should have. "The most important thing in a medical library is its collection of journals and files of current journals. Next to these comes the collection of statistical matters, hospital reports, society reports, collection of cases, vital statistics. The reason for this is that no physician can by any possibility keep these things in his own library. I would say, speaking roundly, that every physician ought to take five medical journals; that every physician who is a teacher or a specialist, or pretends to keep well up with what is going on in his profession, ought to take a dozen journals; that every journal club and small library should take from twenty-five to fifty journals; that every fairly large library, such as this Academy of Medicine, ought to take three hundred current medical journals, and at least seventy-five transactions; every national collection should take the whole lot, good and bad, which numbers considerably over a thousand journals. This number has to be increased by about thirty thousand, in order to complete the files of back medical journals. While recommending as full a list of medical journals and transactions as possible for consultation purposes, I do not wish to underestimate the value of text books and bound volumes. In keeping up the files of journals, hospital reports, circulars of the medical schools, etc., a great deal more labor is required than any librarian can attend to. He must have the assistance of a number of skilled men. I have never before so fully appreciated the influence of a large medical journal as when, some months ago, one in this city stated in an editorial that it was out of the line of an editor's work to look up references and all manner of things for its numerous correspondents, and suggested that they devote ten minutes to writing to the library in Washington, offering a reasonable compensation, when it was probable they would get the information they desired. The number of communications which I received the next few weeks, asking for information on special subjects, was overwhelm-

ing. It was not in the line of duty of the librarian there to attend to such matters, yet with the aid of three resident physicians, who volunteered their services, the information sought for by the correspondents was given for moderate compensation. Every large medical library must be prepared to meet just such emergencies, and an able corps of assistants is required."

The speaker then referred to the labor involved in getting out the "Index Medicus," a publication which Dr. Andrews had said he looked upon with horror. The speaker thought of it with horror also, but without it what hope could there be, eighty years hence, in attempting to look up matter published to-day?

In concluding, the speaker said: "I know it is to the energy, the tact, and the perseverance of a very few men, members of the Academy, particularly the last three presidents, that the success in obtaining this new building is due, and to them is owed a debt of gratitude by the medical profession all over the world."

CONGRATULATIONS FROM PHILADELPHIA.*

By S. WEIR MITCHELL, M.D.

Dr. Mitchell, having been introduced by the President as "the poet, novelist, and distinguished physician," said: "It would seem my friend Dr. Loomis has prepared my audience for a poem, a novel, and a dose of medicine equally. However, I do not intend to administer any of the three on this occasion. I am an honorary member of the Academy, and, therefore, do not come as an entire stranger, but to share in the congratulations of my fellow-fellows, and also to express as a citizen of a neighboring city warmest interest in the welfare of the Academy. I was reminded, on inquiring for a book in the library, of a visit which I once made to the French Academy in Paris, where I desired to find a certain volume which I knew to be among their accumulated riches. I was utterly unable, however, to find it. This great and learned Academy is very badly housed, and without a catalogue for the best library in the world. When asked why they did not have one, I was told that they were waiting for the government to put up a new building. I am happy to say that things are done differently in this country."

The speaker likened the medical profession to a great guild, and related how, when a stranger in Germany, he was taken sick and was doctored by an unknown physician, who, on learning accidentally that his patient was also a physician, refused absolutely to take anything for his

* An abstract of an address delivered on the occasion of the opening of the new building, November 20th, 1890.

services, saying that the year before he had himself been taken care of many weeks while ill in St. Petersburg, by a physician and a stranger who declined any compensation. He added: "You, sir, will pay this fee to some other man whom you will take care of in your far country across the sea."

The speaker would say further that this singular guild possessed a code of morals which was old when Christianity was born, by which we had abided faithfully ever since. It included honor, chastity, brotherhood, and the largest charity. Regarding charity, he added that it had a striking illustration in the fact that men who had attained the highest fame in their profession continued to give from an hour to three hours three or more days of the week to the treatment of the poor, at a time when they could hope to reap no reward whatever from such services. This charity in the profession he thought was not fully appreciated by the public, else they would be more generous in their assistance in building halls and libraries.

Dr. Mitchell then referred to the foundation and development of the College of Physicians of Philadelphia, which had the second largest medical library in this country. A library was never large enough as long as a man on going to it could not find what he wanted. A layman had once asked him why so many books, when most of them contained only matter which had ceased to be of practical use. His reply was that a library was a great museum of facts; it contained matter which must be quoted or referred to by all who wished to write a scientific paper. Let them cease to found libraries and there would soon be a degradation, reducing the profession of medicine to the basis of a useful trade in nostrums.

CONGRATULATIONS FROM BOSTON.

Dr. Reginald H. Fitz, in expressing congratulations from Boston, referred to a branch which might usefully be added to the library of the Academy, namely, a nurses' bureau or directory, like the Nurses' Home, a branch of the Academy in Boston, which had responded to numerous calls the past year.

The President read letters received from L. L. Seaman, Grover Cleveland, Henry I. Bowditch, N. S. Davis, W. H. Welch, and others.

Dr. Oliver Wendell Holmes wrote: "Academies have been too often thought of as places of honorable retirement, and dignity, and ease; nooks where emeritus professors and effete men of letters, once cocks of the walk, could sit in quiet roost while the fighting, the clucking, and the crowing were going on beneath them. No doubt, to be a member of the French Academy—one of the 'forty Immortals'—is an honor worth striving for, in spite of Piron's epigram. But the academy which fulfils its true function is a true working body. It deals with living subjects; it handles unsettled questions; it sets tasks for its members, and furnishes, so far as it can, the appliances required for their prosecution; it offers rewards for meritorious performances, and sits in judgment on the efforts of aspirants for distinction. It furnishes the nearest approach we can expect to a fixed standard of excellence by which the work of new hands and the new work of old hands can be judged. It is a barrier, a breakwater against the rush of false pretensions which are constantly attempting to find their way into public confidence. Nowhere is such a defence more needed

than in the sciences and arts which deal with the health of the community. The public is so ready, so eager to be deceived, and the traders in deception are so willing, so hungry to deceive those who will listen to them, that it needs a solid wall of resistance, a close, united phalanx of men of recognized sense, knowledge, and character, to stand against them.

"The various forms of what I will venture to christen as pseudopathy and pseudotherapy—though they are known to the public by other names—can never loosen the hold of the intelligent thoroughbred physician on the enlightened members of society so long as the best heads in the profession are banded together in a noble institution like this Academy. Only let it ever remain steadfast, unmovable against the recognition of any such pretenders who have no more business in our associations than the astrologist in our observatories or the alchemist in our laboratories.

"We look to this great and able body of men to guard the sacred avenues to the temple of science against all worshippers of idols. The medical profession will always have to fight against the claims of wrong-headed and too often dishonest individuals and 'schools,' as they call themselves. A fraction of every community will always run after false prophets. There are a certain number of squinting brains, as there are of squinting eyes, among every thousand of any population. There will always be a corresponding number of persons calling themselves physicians, ready to make a living out of them. Long may it be before the wholesome barriers are weakened that separate the thoroughbred and truly scientific practitioner from the plausible pretender with his pseudopathy and his pseudotherapy! We trust it will always be enough for a physician to be able to say, 'I am a member of The New York Academy of Medicine.'"

LIFE-SAVING METHODS IN STILLBIRTHS.*

By WILLIAM T. LUSK, M.D.,

PROFESSOR OF OBSTETRICS IN BELLEVUE HOSPITAL MEDICAL COLLEGE, ETC.

A FEW weeks ago I was called in consultation to the bedside of a young primipara in advanced labor. I found upon examination that the child's head was low in the pelvis, and that the cervix was very nearly dilated. The pains seemed good, but for the past two hours there had been no progress of the presenting part. The extraction of the head by means of forceps was a simple task, but after its birth it was found that the advance of the shoulders was arrested by the cord, which was short and drawn tightly around the neck. The cord was, therefore, divided with scissors, and the child was rapidly extracted. It had reached the pallid stage of asphyxia. All respiratory action was suspended, and the heart pulsations had apparently ceased.

The child was placed upon the table and wrapped in warm cloths. The mucus was expelled from the posterior fauces and from the nasal passages by mouth insufflation. A No. 8 English gum catheter was then passed into the trachea, and the aspirated mucus was removed by suction. The quantity of mucus in the bronchial tubes was large, and the catheter had to be introduced many times. After preliminary clearing of the trachea and bronchial tubes direct insufflation was practised. In ten minutes slight heart movements were observed, but these ceased when insufflation was suspended. A little later the child gasped from time to time, usually, it seemed, in response to the irritation of the catheter when introduced into the trachea. An hour's perseverance in this plan of treatment was rewarded by the appearance of a little color upon the surface of the child. The latter was then immersed in warm water, and when lifted from

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* Read before The New York Academy of Medicine, December 4th, 1890.

the bath a stream of spirits was poured from a height upon the epigastrium. No respiratory movements were excited. Schultze swinging movements were next resorted to, and were followed by Sylvester's method; but in employing the latter it was found necessary to draw the tongue forward and to depress the base to secure a passage for the entrance of air into the lungs. It was, however, nearly three hours, spent in the use of warm baths, cold sprinklings to the surface, and in the alternating resort to Schultze's and Sylvester's methods, before spontaneous respiratory movements were finally established. The next day the child had repeated attacks of tetanus, but these finally grew infrequent, and after twenty-four hours ceased altogether. The child is now robust and healthy, and the joy of a worshipping household.

I grant this story is a familiar one, and yet the incident has stimulated me to treat of the subject of life-saving methods in stillbirths. I do this the more earnestly because I believe the attitude of the profession in general is one of incredulity as regards the efficacy of the means at our disposal to restore the life of children in the more desperate cases of asphyxia. In my experience it is the usual procedure to spank the child, to immerse it in hot and cold water, and then to wrap it in warm cloths and place it by the fire to die. Yet the object of medical practice is to save life, and for my part I regard the rescuing of a new-born infant from impending death to be as distinctly a professional triumph as the saving of life by ovariotomy, by Cæsarean section, or by the operation for appendicitis.

To do effective work it is necessary for the physician to make himself familiar with the physiological conditions which occasion the first respiratory act, and the secondary pathological changes which result from suspended animation. For the mechanical resources employed to restore circulation, and to excite spontaneous respiratory movements, should not be selected by divining-rod methods, but should be natural sequences of our knowledge of disturbed conditions calling for relief.

It is, of course, well known that during the period of gestation the child remains in a state of apnœa, and that the respiratory function necessary to its development is performed by the placenta. So soon, however, as the child is born, in normal cases, the thorax expands, the diaphragm contracts, and pulmonary respiration is established. The premature establishment of pulmonary respiration while the child is still in the utero-genital passage, owing to the absence of an atmospheric medium, is followed by asphyxia, and is the usual cause of stillbirths.

The reason of the first respiratory movement in the child, whether prior or subsequent to its birth, has long been a subject for speculation. Omitting earlier views, at present two theories contend for supremacy. The one formulated by Schwartz maintains that in all cases the respiratory act is due to disturbed placental circulation and the consequent lack of oxygen in the blood of the child. Preyer, on the other hand, insists that respiration is a reflex movement provoked by cutaneous stimuli. He admits, however, that a venous condition of the blood favors the action of external stimuli by increasing the irritability of the respiratory centres.

As a contribution to the solution of the questions in dispute, Otto Engström* has recently reported a series of experiments made by him upon gravid rabbits and guinea-pigs in Preyer's laboratory. The animals were strapped to a board, and were then immersed in a saline solution (six per cent.), which was kept at blood heat by a special apparatus. The head of the animal was placed above the fluid. A small incision was then made in the abdominal wall, through which a uterine cornu was allowed to escape into the saline fluid. The uterine walls were next opened opposite the mesenteric attachment at the point of least vascularity. As there was no hæmorrhage to stain the saline fluid, it was possible to observe the fœtuses through the membrane in the clear amniotic fluid.

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* "Ueber die Ursache der ersten Athem bewegungen."

When this experiment was performed with address and dexterity, the exposed fœtus remained in a state of apnœa, and the blood in the umbilical vein possessed a bright-red color, in marked contrast with the dark hue of the blood in the umbilical arteries.

If now the cord was compressed through the membranes by the thumb and index finger of the warmed hand, or by self-closing compressing forceps applied to the cord near to the placenta, and as far from the fœtus as possible, respiration followed in from three to six seconds, and continued until death supervened from asphyxia. The same results followed when the umbilical vein was pricked with a needle or was divided by scissors. Again, in other cases, to avoid the criticism that external stimuli were not absolutely excluded by the manipulations employed, the mother was asphyxiated by carbonic acid gas, or poisoned by woorari, or bled to death by opening the carotid. Here, too, soon after the blood in the umbilical vein became of a venous hue, respirations occurred as heretofore, though all manipulations were carefully avoided. It is, therefore, demonstrated that fœtal respirations are excited, in the absence of external sources of irritation, so soon as the blood in the umbilical vein becomes darkened or is cut off from the fœtus.

On the other hand, in another series of cases, where the amniotic sac was exposed under a blood-warm saline fluid, and the apnœa was not disturbed, Engström gently pricked the extremities of the fœtus with a needle. Reflex movements were excited, but the apnœa continued. When, however, deep puncture was made, the alæ nasi dilated, the mouth opened, and thoracic inspiration was evoked. The effect was, however, momentary, and the apnœa returned.

Again, in order to reduce the disturbance of the placental circulation to a minimum, Engström, following a method invented by Preyer, seized the head of a fœtus with the thumb and index finger through the abdominal walls, and then cut through the abdominal coverings, the

uterine walls, and amniotic sac to the nose of the animal. The nose was then lifted above the saline fluid and exposed to the atmosphere. By means of powerful currents of induced galvanism applied to the nasal organ, respiratory movements were excited, but ceased when the current was removed. It seemed doubtful, however, whether inspiratory acts did occur in either series of experiments so long as the placental circulation was completely undisturbed. The existence of apnœa in the fœtus is not conclusive evidence that the opening of the uterus or the compression of the uterine walls produces no derangement in the blood currents of the placenta. The results were often negative at the beginning of the experiment, but as the blood in the umbilical vein darkened, the electric and mechanical irritants produced more and more marked effects.

From these experiments it becomes evident that when the placental respiration is suspended the accumulation of unknown materials in the blood is capable of exciting the respiratory centre in the medulla oblongata of the fœtus without the aid of peripheral stimuli; but that the latter are capable of exciting the respiratory act before the internal stimuli have increased sufficiently to induce independent action. Again, it is a familiar fact that in moderate degrees of asphyxia in the new-born, after the irritability of the medulla has been lowered to a point at which no response follows from the venous condition of the blood, external stimuli are still capable of exciting respiratory movements.

Engström found, too, that when the fœtus had breathed in the amniotic sac, after respirations had ceased, and after the blood in the umbilical vein and arteries had become of the same blue color, and the nose and lips had become cyanotic, it was still possible to excite respirations in some cases by opening the amnion and lifting the head so as to expose it to the air, and in others by pinching the nostrils, the ears, and the mouth.

I shall hardly need to apologize for endeavoring to

recall to the memory of my readers the peculiarities of the fœtal circulation. The arterialized blood in the umbilical vein empties partly into the portal vein and is first distributed to the liver, and in part passes by the ductus venosus into the inferior vena cava. The mingled venous and arterial currents then enter the right auricle, and are in early pregnancy directed by the Eustachian valve across the right auricle to the left auricle, and thence pass to the left ventricle. As the heart contracts the blood is driven from the left ventricle to the aorta, and is thence distributed by the large vessels which spring from the latter to the head and upper extremities. The blood returned from the upper portion by the superior vena cava, enters the right auricle, where it passes in front of the Eustachian valve to the right ventricle. With the advance of gestation, however, a gradual disappearance of the Eustachian valve takes place, so that a part of the blood from the inferior cava enters with that of the superior cava into the right ventricle. The contraction of the right ventricle forces the blood into the pulmonary artery, which distributes an insignificant quantity to the lungs, while the main current passes through the ductus arteriosus to the aorta, by which it is distributed to the lower portion of the body. Now, though the greater part of the regenerated placental blood is distributed to the head and upper part of the body, it is mingled largely with venous blood returning from other organs, and that which goes to the respiratory centre in the medulla oblongata is of a character which would cause dyspnœic manifestations in self-breathing individuals.

During labor, especially in the last stage, the placental aëration of the blood is interfered with by the uterine contractions, and in its passage through the pelvis the surface of the child is subjected to pressure and friction. At birth the body is exposed to the air. These combined causes as a rule are followed by pulmonary respiration, though, in some instances of lowered irritability of the

medulla, prolonged apnœa follows the birth, unless the child is made to cry by vigorous slapping.

As the chest expands in the act of inspiration the lungs fill with air, and the blood from the pulmonary artery pours into the opened pulmonary vessels. The pressure in all the vessels of the body is diminished, though in the thorax a partial compensation takes place from the aspiration of blood from the veins which enter the intra-thoracic space. The diminution of pressure is greatest in the pulmonary artery. The current which empties into the aorta becomes of feeble force, and finally ceases altogether. The ductus arteriosus gradually closes, and the pulmonary circuit becomes complete. The withdrawal of the blood from the pulmonary artery, and the force of aspiration, lower the tension in the aorta. The heart beats more slowly. The resulting diminished arterial pressure is most felt in the extremities. The pulsation of the umbilical arteries as a consequence ceases, and the placental circulation is suspended.

In the asphyxia of new-born infants the suspended animation is, with few exceptions, preceded by intra-uterine respirations. The causes of the latter are to be found in tetanic contractions of the uterus and the consequent diminished blood supply to the placenta, in premature detachment of the placenta, in compression of the cord, and in the sudden death of the mother. Of these the compression of the cord is by far the most common.

The first effect of the compression of the cord is to arrest the circulation in the umbilical arteries. The pressure in the aorta is thereby augmented, and increased work is thrown upon the left ventricle of the heart. Except in cases where the mouth and nasal passages are closed by pressure, with the expansion of the chest, due to the irritation of the medulla by the increased venosity of the blood, amniotic fluid, meconium, and mucus are aspirated into the air passages. When the compression of the cord is temporary the circulation may be restored and the apnœa may again return; but in cases where the

respirations continue, the capillaries of the lungs fill with blood from the pulmonary artery, the intrathoracic venous congestion is increased, and the heart action is lowered. As the irritability of the medulla sinks, the respirations fail, the cavities of the heart fill with venous blood, the lungs are congested, and in some instances subpulmonary ecchymoses result from overdistention of the pulmonary vessels. Outside the thoracic cavity the venous trunks are often distended with blood. This secondary venous stasis is most marked in the vessels of the neck, head, and brain, but to a less degree venous stases are likewise observed in the abdominal organs and in the capillaries of the skin.

After the birth of the child the prognosis is dependent upon the degree of asphyxia. In the milder cases the muscular tonus is preserved, the head does not drop, the extremities are not relaxed, the skin is dusky-red or cyanotic, the conjunctivæ are injected, the umbilical vessels are distended, and the cardiac and umbilical pulsations are slow but forcible. Reflex movements are easily produced by surface irritation. In these cases spontaneous respiratory movements often occur.

In the more advanced stage the muscular tonus is lost, the head falls to one side, the sphincters are relaxed, the eyes are glazed, reflex movements cannot be provoked, the heart beats are feeble and infrequent, the umbilical vessels are nearly empty, and the pulse scarcely perceptible. The cutaneous circulation has disappeared, and the surface is pale and cold.

If respirations occur, they are feeble and are not associated with the movements of the muscles of the face. The first signs of returning animation are the refilling of the cutaneous capillaries and the restoration of the muscular tonicity. In fatal cases the pulmonary vessels are found to be widely distended, the lungs are heavy and of a dark-red color, and pulmonary, subpleural, subpericardial, and subendocardial ecchymoses are present. The obstruction

of the pulmonary circulation further produces congestion of the abdominal viscera and of the brain.

The indications for treatment are in all cases to clear out the air passages, to restore the irritability of the medulla, to increase the force of the heart contractions, and to relieve the plethora of the heart and of the blood channels of the thorax.

In cases where the muscular tonus is preserved these indications are, as a rule, easily fulfilled; aspirated fluids and mucus should be cleared from the fauces with the finger. If the nasal passages are obstructed, mouth-to-mouth insufflation should be employed. The child should be made to cry by flagellation, and the respiratory movements should be further stimulated by alternately immersing the child in hot and cold water. So far the procedure is a familiar one, but in a good number of cases we know that in a few days the skin becomes dusky, the heart action feeble, and the child has been temporarily restored to life only in the end to die of atelectasis. As a means of guarding against this fatal consequence, due in part to imperfect expansion of the lungs, in part to lobular congestion, there is no method that rivals the one of Schultze.

Schultze directs that the child should be grasped in such a manner that the operator's thumbs rest, on either side, upon the anterior thoracic wall, while the index finger occupies the axilla and the remaining fingers are placed diagonally across the back. The child is then allowed to hang at arm's length between the knees of the obstetrician, its face being turned to the front. In this position the pectoral muscles are made to draw the superior ribs upward, the abdominal muscles draw the inferior ribs downward, and the weight of the liver causes the descent of the diaphragm. By this means the capacity of the chest is increased and inspiration is produced. The child is next swung upward until the arms of the operator reach an almost horizontal position. The swinging motion is then arrested, flexion occurs in the child's lumbar spinal region, its head is directed downward, and its lower ex-

tremities fall slowly toward the obstetrician, until the whole weight of its body rests upon his thumbs. In this way the chest and abdomen are powerfully compressed, the diaphragm is forced upward, and an efficient expiration results, and any retained adventitious matters are expelled from the air passages. An inspiration is now produced by reversing the direction of the swing and returning the child to its former position of complete extension, by which manœuvre the chest is made to expand and the diaphragm to descend.

By this method not only is good aëration of the lungs secured, but the forcible expiration expels the materials aspirated from the bronchial tubes. A still more important action, according to Schultze, is the relief of the overloaded vessels as a result of the compression of the entire thoracic contents. Thus, as expiration is produced by the upward swing, the blood is pressed from the left ventricle into the aorta, and from the right auricle into the right ventricle. The emptying of the left ventricle makes room for the contents of the left auricle and permits the return current from the pulmonary veins. From the right ventricle the surplus blood finds a passage into the aorta through the ductus arteriosus. With the inspiratory swing blood is aspirated from the peripheral vessels into the blood channels of the thorax. The aspirated blood is, however, venous, as the semilunar valves prevent regurgitation from the aorta. By alternating the expiratory and inspiratory swinging movements the pump-working of the heart is mechanically set in action. As the blood streams pass through the heart cavities the systole increases in force and the arterial tension is restored.

In cases of deep asphyxia, in which muscular tonicity is lost and the heart movements are scarcely perceptible, the methods at first employed should involve the minimum degree of disturbance to the child. Active movements are, as a rule, speedily followed by the extinction of heart pulsations.

The child should be laid upon a table and covered

warmly. After clearing the fauces and nasal passages, a No. 8 English elastic catheter should be passed, under the guidance of the fingers of the left hand, through the larynx into the trachea, and aspirated matters should be carefully removed by suction. This often requires time and the repeated introduction of the tube. Meantime, at intervals, insufflations through the tube into the bronchial tubes should be employed. After each insufflation the chest walls should be compressed with the hand to produce expiration. By this means, little by little the blood receives oxygen, and the returning irritability of the medulla is manifested by occasional spontaneous respiratory movements. Then the Sylvester's method, which Champneys has shown is followed by the fullest expansion of the air cells, is of service. Only, to secure favorable results, it is often necessary to draw the tongue well forward. So soon as the heart movements become plainly perceptible, the child should be placed in warm water, and sprinklings of cold water upon the face should be practised. Finally, the swinging method of Schultze should be employed to complete the establishment of the normal circulation.

But in these cases of deep asphyxia no miracles are to be wrought without patience, watchfulness, and a hopeful spirit. Often, when a certain amount of progress has been obtained, the heart action begins to die out, and we find ourselves obliged to go back to the catheter and insufflations anew. There is, however, no greater joy to the physician than that which comes from watching the gradual restoration to life of a young child, and to know that the life has been saved by the intelligence and the knowledge which God has given to us.

ORBITAL OPTIC NEURITIS, INCLUDING ALCOHOL AND TOBACCO AMAUROSIS.*

By H. KNAPP, M.D.

THE singular cases in which people notice a haze, a blur, even a perfectly dark patch, in the centre of their field of vision, known under the name of central amblyopia or central scotoma, as well as those cases in which persons lose their sight in one or a few days without exhibiting symptoms of any ocular or general disease to account for this appalling occurrence, were, more than twenty years ago, accounted for by A. v. Graefe † and Theo. Leber ‡ by the supposition of an inflammation in the orbital part of the optic nerve—*retrobulbar neuritis.* By anatomical investigations during the last ten years this hypothesis has been proved to be a reality, as we shall see further on.

Retrobulbar neuritis may be idiopathic or the result of different kinds of intoxication, alcoholism, nicotinism, lead poisoning, diabetes, syphilis, etc. It has an acute and a chronic form.

(*a*) ACUTE RETROBULBAR NEURITIS.

SYMPTOMS.—1. *Headache*, in various degrees, sometimes quite severe.

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* Read before The New York Academy of Medicine, December 18th, 1890.

† "On Neuro-Retinitis and Certain Cases of Fulminant Blindness." His Archives, xiv., 2, pp. 114–149 (see particularly pages 146–149), 1866.

‡ "Amblyopia with Central Scotoma." V. Graefe's Arch., xv., 3, p. 65, etc., 1869; and "Gräfe-Sämisch's Handbuch," vol. v., p. 809, 1876.

2. *Orbital pain*, increased by movements of the eye and pressure upon it.

3. *Impairment of sight*, in all degrees. It may begin without any warning and advance rapidly, leading, in exceptional cases, to total blindness of both eyes in a day or two.

4. *Central scotoma*, for color and form. It may be of little intensity or so marked that no object is recognized in its area—partial and complete (or absolute) scotoma. The peripheric boundaries of the visual field remain normal.

5. *General diminution of color perception.*

6. *Moderate retinal congestion and serous effusion* of the optic disc and adjacent retina. This condition may be followed by *ischæmia*, or the latter may be noticed from the beginning. The congestive swelling is to be considered as the distal boundary of the inflammatory focus, whereas the ischæmia is the result of the compression of the retinal vessels by the inflamed parts in the orbit.

7. The *termination* reveals three ophthalmoscopic pictures:

(*a*) Normal condition.

(*b*) Partial atrophy—always in the temporal half—of the optic disc.

(*c*) General atrophy of the optic nerves.

ETIOLOGY.—1. *Exposure and overwork.*

2. *Acute infectious diseases, e.g.*, measles, diphtheria, rheumatism, etc.

3. *Alcohol, nicotine, opium, lead poisoning, etc.*

4. *Suppression of menses.*

5. *No assignable cause.*

PROGNOSIS.—Doubtful. We find complete and permanent recovery and partial recovery of sight, as well as permanent blindness, sometimes with preservation of one or several islets in the visual field, like oases in the desert. Their explanation later on.

TREATMENT.—Rest, diaphoresis, salicylate of soda, mercury, iodide of potassium.

Let me detail one *illustrative case* from my own practice.

May 5th, 1883, I was called to see Mr. Nathan Steinam, of New York City, forty years old, a successful merchant, healthy, without a constitutional taint, with no bad habits. The previous day he had complained of nausea and severe headache. In the evening his sight was dim; the next morning he was totally blind in both eyes. I found S = 0, both pupils large, immovable; eyeballs of normal tension, their movements undisturbed. The media clear, the retinal arteries small, the veins enlarged and tortuous, the retina in the vicinity of the disc turbid, *i.e.*, a slight degree of congestive neuro-retinitis. The patient was a well man, excepting his binocular blindness, and that he passed only a few ounces of urine during the day. The two next days the same condition. The urine drawn with a catheter, still scant, but free from albumin, casts, and sugar. On the fourth day he passed more urine. Eyes the same, arteries more filled, but no pulsation on pressure; veins dark, like thrombosed. Dr. Wiener, his family physician, Dr. Wm. H. Draper as consultant, and myself thought that the patient suffered from a uræmic attack. On the seventh day he had a spell of somnolence, but recovered soon and felt well. The excretions of urine became normal, the retinal arteries filled gradually to about two-thirds of the natural calibre and pulsated on pressure, the veins lost their turgescence, the retina became transparent, and in about six weeks the optic discs were white, distinctly atrophic. Seven weeks after the onset of blindness he had, at various hours of the day, transient glimpses of light, during which he recognized the window and a lamp, both of which he correctly localized. When I came to see him I never could verify these observations, and was inclined to consider them as subjective sensations of light; yet the patient, a man of well-balanced mind, positively asserted that he had seen light, though it disappeared quickly. He was treated with salicylate of soda, calomel *refracta dosi*, strychnia hypodermically, large doses of iodide of potassium, electricity. Dr. A. L. Loomis, in consultation, was of the opinion that some cerebro-spinal lesion would sooner or later manifest itself.

Thus far, almost eight years, no general disease whatsoever has appeared. The patient is physically and mentally as well as any person of his age. He goes regularly to his business, and is an intelligent and cheerful man, resigned to his loss of sight. But what has appeared is an

islet of useful vision nasally and downward from the point of fixation in the visual field of the right eye, in which he counts fingers at a distance of 15′. Yet he takes in only three fingers at a glance, and has slightly to move the eye to see the others. This represents an angle of a little over 1°. His optic discs are perfectly white, the blood vessels about two-thirds the normal calibre. The arteries pulsate on pressure. The picture resembles that of atrophy after total embolism of the central retinal artery. On examination, a few days ago, the patient's general and ocular conditions were found unchanged.

(*b*) CHRONIC RETROBULBAR NEURITIS.

SYMPTOMS.—1. *Diminution of sight*, binocular, gradual, yet in some cases with acute aggravations.

2. *Day blindness*, patients see better in the dusk, the disease first described by Arlt under the name of nyctalopia. The patients complain of a fog and are dazzled; at night they feel more comfortable, but in reality see no better.

3. *Diminished color perception*, contrast shadows not distinguished.

4. *Central scotoma*, at first for colors, then for forms (absolute). Ring scotoma, by restitution of the central part.

5. *Diminished range of accommodation.*

6. *Boundaries of visual field normal.*

7. According to the stage of the disease, we find *ophthalmoscopically:*

(*a*) Fairly normal condition or slight congestion.

(*b*) Sector-like atrophy in the lower outer part of the optic disc.

(*c*) General atrophy of the od.

OCCURRENCE.—Almost exclusively in males, and in the great majority of cases from abuse of *alcohol or tobacco*, or more frequently *of both.* Opium, stramonium, lead, carbon sulphide, malaria, syphilis, diabetes, rheumatism, and gout are mentioned as causes. Of late cases have been reported as sequels of epidemic influenza.*

* Bergmeister, Wien. klin. Wochenschr., 1890, No. 11. Landsberg, Centralbl. f. Augenh., May, 1890, p. 141. Remak, ibidem, July, p. 201. C. M. Hansen, West. Med. Reporter, October, 1890, p. 231. Eperon, Progrès Méd., December, 1890, p. 471.

PROGNOSIS good in the first stage, when no atrophy of od. is present. Rarely ending in complete blindness. The triangular atrophy of the od. may persist, but sight become normal again. Incomplete recovery is the rule.

TREATMENT.—Insist on total abstinence from alcohol and tobacco. Order strychnia, iodide of potassium, Turkish baths, pure and invigorating air—sea, mountain, forest. Strychnia hypodermically, gr. $\frac{1}{30}$ or more, acts as an excitant and a tonic, with transient improvement of vision.

DIAGNOSIS OF CASES OF RETROBULBAR NEURITIS.

Ibeg to make a few remarks about the *examination of such patients.* The majority complain of impairment of sight. With test types at a distance we find both eyes affected, S from $\frac{20}{30}$ down to counting figures at several feet, eccentrically.

Near by they require stronger glasses than their age and amblyopia would warrant.

Their field of vision has normal boundaries.

With the O. S. we notice one of the three stages: no abnormity, atrophy on temporal side, or general atrophy of the optic disc.

Examining for color perception, we find that the patients of the last category are color-blind, those of the second recognize pigments well enough, but fail in contrast shadows; all of them show a color scotoma. The best mode to find this out is to hold a small piece of colored paper, not more than three millimetres long, with a pair of forceps in the visual line, while you look at the patient's eye and let him look at yours. He will either be unable to see the color, especially green or red, or will confound it with another. If you want to measure the extent of the scotoma, the handiest instrument is the perimeter of Schweigger. We shall, of course, not omit to examine the patient's general condition as to the well-known symptoms of alcoholism and the other diseases mentioned before.

PATHOLOGICAL ANATOMY.

Von Graefe in 1866, and Leber in 1869, found the pallor in the temporal half of the od., and pronounced it to be the result of retrobulbar neuritis. Leber felt confident that the anatomical proof would soon be furnished. This was done in 1880 by Samelsohn* of Cologne, and Nettleship and W. Edmunds† of London. These investigators ascertained that in a limited area of the optic nerve there were augmentation of nuclei, hypertrophy of connective tissue, and waste of nerve fibres, *i.e.*, *interstitial*, *sclerosing inflammation*, in the same way as alcohol produces cirrhosis of the liver.

Following up the process from the eye toward the brain, they discovered that the degenerated segment near the eye had the shape of a triangle, with its base on the lower outer margin of the nerve, its apex at the central vessels. Toward the brain it gradually passed from the periphery of the nerve to the centre, which it reached in the optic canal. A case of Vossius,‡ and one of Sachs,§ confirmed these investigations. The authors followed the atrophic segment into the chiasm and the optic tracts.

The white sector in the od. seen with the O. S. is, of course, the direct observation, *intra vitam*, of the atrophic segment. I have repeatedly seen it, and pass a number of sketches around. By these investigations the course of the papillo-macular fibres through the optic nerve is traced, and we know now what is the lesion in central amblyopia from alcoholism and kindred affections. Some years ago, after my attention had been directed to this triangular optic atrophy in alcoholism, etc., I was astonished to find it clear and well marked in one eye of a healthy and absolutely temperate girl of eighteen years of age. On further examination I noticed that there was,

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* Graefe's Arch., 1882, No. 1, pp. 1–109.

† Trans. Ophth. Soc. Un. Kingdom, 1881, p. 124, etc.

‡ Graefe's Arch., xxviii., 3, p. 301.

§ Graefe's Archives, 1889, p. 133.

in that eye, *a coloboma of the macula lutea.* Since that time I have found the same in other cases of macular coloboma. This affection, being either a congenital defect or the result of choroidal hæmorrhage or circumscribed exudation, gives, according to Gudden's law of ascending atrophy, a neat confirmation of the position of the macular fibres in the optic-nerve entrance. In a recent paper by G. Lindsay Johnson,* of London, on extra-papillary colobomata, which is illustrated by excellently executed colored drawings, there are several cases of coloboma of the macula lutea which show this triangular atrophy of the od.

An excellent paper by Dr. W. Uthoff, of Berlin, appeared, four years ago, in *Graefe's Archives*,† on the "Influence of Chronic Alcoholism on the Human Visual Organ." Uthoff utilizes a clinical material of thirty thousand patients (Schöler's Klinik), and has examined, in different asylums of Berlin, one thousand inebriates functionally and ophthalmoscopically. He found the triangular white atrophy of the od. in fourteen per cent. Of six fatal cases he received the eyes and examined them with the microscope most thoroughly. I pass his plates around, exhibit some with the magic lantern, and am happy to show you under the microscope some of his specimens which he had the kindness to present me with. The one shows the central interstitial neuritis in the period of nuclear infiltration, the other in the stage of connective-tissue hypertrophy. The process is characteristic by the limitation of its area of inflammation, circumscribed and triangular near the disc, of which we gain a view in the living with the ophthalmoscope, then oblong and round as it ascends toward the optic foramen. In one of the lantern specimens you see the changes pass like a band from the temporal border of the nerve straight across to the nasal border. This shows how the atrophy of the nerve may become total.

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* Graefe's Archives, 1890, pp. 1–29.

† Year 1886, part iv., p. 95, and year 1887, part i., p. 257.

It is remarkable that in all the specimens a certain number of healthy nerve fibres are seen preserved in the atrophic parts. This explains on the one hand why alcoholists rarely become totally blind, and on the other why, in the visual field of persons blind from retrobulbar neuritis, islets of useful sight may permanently be preserved—oases in the desert, as I called them in the case reported before.

Retrobulbar optic neuritis presents an example of peripheric neuritis of great scientific and practical importance, the nature of which, correctly foretold by exact clinical analysis, has of late been verified in the most gratifying manner by pathologists. The process may start at different points of the nerve. Samelsohn found in his case the origin in the optic canal; in two of Uthoff's cases the changes in the nerve reached the optic foramen and entered the cranial cavity; in the four others they were present only in the distal end of the orbital portion of the nerve, at its entrance into the eye, and from six to twelve millimetres back. In all of these cases the temporal pallor of the od. was seen with the ophthalmoscope. In the fatal cases in which the ophthalmoscope showed no changes, none were found post mortem.

PAPILLITIS, OR INFLAMMATION OF THE INTRA-OCULAR END OF THE OPTIC NERVE:

ITS ÆTIOLOGY AND CONNECTION WITH INTRACRANIAL DISEASE. *

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The Optic Disc or Papilla; the Intra-ocular End of the Optic Nerve.—The intra-ocular end of the optic nerve, as seen with the ophthalmoscope, is the only nerve in the body which is visible to the eye. The nerve fibres on entering the eye radiate and spread out in and upon the retina, but not equally in all directions. They are more numerous on the nasal side, and above and below, than they are on the temporal side. The minute nutrient vessels of the disc are derived partly from the short posterior ciliary arteries and partly from the central retinal artery, twigs from both of these sources uniting to form the so-called "circle of Haller," which surrounds the optic nerve behind the disc. The nerve enters the sclerotic through a funnel-shaped opening, the wider part of which is directed backward. The central depression in the disc is called the "physiological cup" or excavation, and its size and depth are determined by the arrangement of the nerve fibres. Its color is usually paler than the rest of the disc. The boundary of the disc is the "choroidal ring," the edge of the opening in the choroid corresponding to that in the sclerotic. As the latter is the

* Read before The New York Academy of Medicine, December 18th, 1890.

smaller of the two, a narrow rim of sclerotic is seen within the "choroidal ring," which is known as the "sclerotic ring." The trunk of the optic nerve has a double sheath. The inner sheath is delicate, closely surrounds the nerve, and is continuous with the pia mater of the brain. The outer sheath is thick, dense, fibrous tissue, which merges into the sclerotic, and at the optic foramen is continuous with the dura mater. The vaginal space surrounding the optic nerve is continuous with the subarachnoid and subdural spaces around the brain. As the nerve enters the eye, the vaginal space passes inside the posterior part of the sclerotic opening. This space is here, according to some authors, closed, while according to others it is continuous with the lymphatic space in the optic nerve and retina.

There are certain pathological changes occurring in the eye which are common to both optic nerve and retina. When the retina is inflamed the optic nerve is very apt to participate in the process. But the disc also undergoes inflammatory changes independently of the retina. These pathological changes are of two kinds: increased vascularity usually associated with swelling, and diminished vascularity commonly accompanied by shrinking. The former are more or less inflammatory, and are sometimes, though wrongly, classed together as "optic neuritis." In speaking of the inflammatory changes occurring in the intra-ocular end of the optic nerve, I shall employ the term "papillitis," as first proposed by Leber, in order to avoid confusion.

The signs of increased vascularity or inflammation of the papilla consist usually of several more or less distinct stages: 1. There is simple congestion or abnormal vascularity, characterized by simple redness, without swelling or obscuration of the end of the disc. 2. Congestion with œdema, slight papillitis, characterized by redness, swelling, and more or less complete obscuration of the edge of the disc. 3. True papillitis, characterized by greatly increased redness and swelling of the disc, with total ob-

scuration of the edge of the disc, and often of the entire disc itself.

Simple Congestion.—A few words in passing in regard to simple congestion. Increased redness is the usual symptom of tissue hyperæmia, but it is of much less value as a sign of hyperæmia of the optic disc than in the case of other tissues, on account of the great variation which may exist in the amount of normal redness. The apparent tint of the optic disc is a very frequent source of error in ophthalmoscopic examination of eyes by unskilled observers, and its importance is greatly overestimated. It is significant only when it presents special characteristics, or when developed under repeated examinations, or when it is more pronounced in one eye than the other. Gowers is right when he advises that the term congestion or hyperæmia of the disc is best restricted to those cases in which the increased vascularity exists alone without any demonstrable change in the appearance or distinctness of the edge of the disc.

Papillitis.—Congestion with œdema may be regarded as the first stage of papillitis. The normal pink tint of the disc becomes increased, the edge of the disc is blurred, and there may be a more or less distinct halo surrounding it. The centre of the disc may be much redder than the periphery. The vessels may be normal or the retinal veins may be enlarged. These appearances are said to be the effect of pressure on the retinal vein, causing passive congestion of the retinal vessels, but they may occur without the least sign of mechanical congestion.

From congestion with œdema to actual inflammation the transition is one of degree alone. The term "papillitis" should be restricted to those cases in which the swelling, redness, and opacity are sufficient to conceal the edge of the disc. The changes here observed are due not merely to increased congestion and œdema, but to changes in the optic-nerve fibres and surrounding connective tissue. The different appearances on the disc in different cases, and in the same case at different times, have led to the

introduction of two terms to describe different varieties of inflammation—viz., first, "descending neuritis," and, second, choked disc. In true "papillitis" we have proliferation of nuclei, escape of leucocytes into the connective tissue, and finally degeneration of nerve structures. As the swelling of the disc increases, white lines and spots on the disc are not uncommonly seen, often corresponding to the position of the arteries. Hæmorrhages, usually small, are not uncommon. The arteries show little change, but the veins become dark and tortuous, and sometimes dilated. In the early stages of papillitis from tumor they are said to be less frequently dilated than in papillitis from meningitis. As the papillitis goes on, the swelling increases, and often becomes so great that there may be great difficulty in seeing the disc even with a convex glass, and thus the disc becomes markedly hypermetropic. The veins are darker, more dilated, and the arteries are narrowed. Both arteries and veins may be concealed by the œdematous swelling. The swelling may invade the adjacent retina in all directions for a long distance, and occasionally white patches of exudation are seen, surrounded by small hæmorrhages. It should not be forgotten that papillitis in this stage may disappear completely. A further increase in the inflammatory process is always accompanied by signs of compression and strangulation of the papillary vessels. In this stage the arteries are very much narrowed, hæmorrhages are numerous and scattered over the entire fundus, the veins are distended and tortuous as far as the periphery, and the retina becomes more or less opaque.

An inflammation of the disc, or papillitis, may remain for weeks, or months, or even years, in the lower degrees unchanged, or the most intense strangulation of the vessels and disc may come on in a few weeks. All subjective symptoms may be entirely absent even when papillitis is present, the vision being unimpaired and the field of vision being unrestricted. Photophobia, or intolerance of light, and pain are usually rare. In the more severe cases of

papillitis the sight is always impaired and may be entirely lost. The impairment of vision usually occurs earlier in one eye than in the other, and it may come on slowly or rapidly, but almost never suddenly. A narrowing of the field of vision usually accompanies any marked loss of vision, and very often the defect in the field is very irregular. In some cases a defect in the field due to intracranial disease, such as hemianopsia or central scotoma, may accompany the peripheral limitation of the field due to the papillitis. This peripheral limitation is sometimes very marked. Another symptom to be carefully noted is a defect in the field for colors, and this may exist even when the vision is unimpaired. The perception for red and green is usually lost before that for yellow and blue.

The papillitis occurring in the course of intracranial disease may be accompanied by defective vision, due not to intra-ocular changes, but to a lesion in the course of the optic-nerve fibres or their nuclei of origin. Some authors are of the opinion that loss of vision following intracranial disease is more frequent in cases of descending neuritis than in cases of isolated papillitis. This may be true, but it should not be forgotten that it is an exceedingly difficult matter to distinguish ophthalmoscopically between papillitis due to descending neuritis and purely isolated papillitis.

Loss of vision from isolated papillitis never occurs suddenly, but we do meet with it occasionally from brain disease. A symmetrical hemianopsic defect in the visual field points to an intracranial cause; and an unsymmetrical defect, especially a loss of the temporal halves of both fields, usually indicates a pressure on the chiasm from dis tention of the third ventricle. Complete loss of the sight of one eye and defect of the adjacent half of the field of the other eye are probably of cerebral origin. A peripheral narrowing of the field of vision is usually referred to a lesion in front of the chiasm.

Causes of Papillitis.—The most common cause of papillitis is intracranial disease; and of these intracranial

lesions, tumor is much the most frequent. The frequency and severity of the papillitis do not seem to be materially influenced by either the nature, the size, or the location of the tumor. The next most frequent cause is meningitis; then follow abscess of the brain, hydatid disease, and softening from thrombosis or embolism, in the order named. Papillitis is also occasionally met with in acute diseases of the spinal cord. The diseases of the general system which sometimes cause papillitis are chronic Bright's disease, certain febrile disorders, anæmia from loss of blood, etc. Papillitis may also occur as an idiopathic lesion, or from great menstrual disturbances, or from exposure to long-continued and severe cold. Unilateral papillitis is almost always due to a lesion in the corresponding orbit.

The *duration* of papillitis varies widely. It may reach its height in a few weeks and then subside, or it may be so chronic that months and even years elapse without the slightest apparent change in the ophthalmoscopic picture.

The Relations of Papillitis to Intracranial Disease.—The first definite theory of the method in which intracranial disease acts in causing papillitis was advanced by Von Graefe in 1859, and still further elaborated in 1866. It was based on certain cases of papillitis, with hæmorrhages, occurring in the course of cerebral tumor, in which no signs of inflammation were demonstrable macroscopically in the trunk of the optic nerve; whereas, in certain other cases of meningitis in which the ophthalmoscopic changes had been less marked, inflammation was found in the course of the nerve trunk, and this was assumed to have descended from the inflamed meninges. This condition was called "descending neuritis" by Von Graefe, who contended that the ophthalmoscopic characteristics were a slight degree of change in the color and swelling of the disc and a tendency to invade the adjacent retina. The cases of brain tumor with marked papillitis and hæmorrhages, but with no evidence of inflammation in the nerve trunks, he explained by the theory that they were due to

the effect produced on the circulation of the eye by the increased intracranial pressure, which obstructed the return of blood from the eye by compressing the cavernous sinus. This mechanical effect was greatly intensified by the unyielding character of the sclerotic ring. To this combined swelling of the disc with hæmorrhages and vascular distention he gave the name "*Stauungspapille*," which we translate as "choked disc."

This "obstruction" theory was accepted as more or less satisfactory until 1869, when Sesemann dealt it the first severe blow by demonstrating that the communication between the orbital and facial veins was so free that the effect of pressure on the cavernous sinus was at once relieved, and the latter caused merely a very transient engorgement of the retinal veins. He also proved that even obliteration of the cavernous sinus produced scarcely any ophthalmoscopic change in the appearance of the disc.

In 1869 Schwalbe discovered that the subvaginal space around the optic nerve is continuous, at the optic foramen, with the subdural spaces around the brain, and could be injected from them. These facts agreed with previous observations which had been made by Stellwag in 1856 and Manz in 1865—that the sheath of the optic nerve might be distended in optic neuritis caused by tumors and meningitis.

In 1869 Schmidt suggested that the intracranial pressure might influence the intra-ocular end of the optic nerve by this mechanism, since the distention of the sheath is usually greatest just behind the eyeball. This theory was supported in 1871 by Manz, who showed how frequently distention of the sheath was met with in optic neuritis, and who believed it to be an invariable occurrence in cases of intracranial pressure or increase of the subarachnoid fluid. He thought that the simple pressure on the nerve and blood vessels might cause the intra-ocular changes, and he endeavored by experiments on animals to demonstrate this effect of the vaginal distention.

Injections into the subdural space passed into and distended the sheath and caused engorgement of the retinal veins, and in some cases a transient redness and swelling of the papilla.

Schmidt found, however, that a colored liquid injected into the sheath passed into the lymph spaces in the nerve at the lamina cribrosa, and therefore suggested that the neuritis was produced, not by the simple pressure outside the nerve, but by the irritating influence of the liquid which passed into these lymph spaces. These theories of Schmidt and Manz have been generally accepted, at least in Germany, as affording the most satisfactory explanation of the origin of optic neuritis.

In 1881 Leber adopted the view that the distention of the sheath is the immediate exciting cause of neuritis, but he doubted the "mechanical-pressure" theory of Manz, and rejected absolutely the "irritation" theory of Schmidt. He suggested that the fluid in the sheath excites neuritis by conveying pathogenic material to the optic nerve behind the eye, and Deutschmann, in 1887, published some experimental evidence in favor of Leber's view and in opposition to the theory of the causation of "choked disc" by distention of the nerve sheath.

In 1863 Hughlings Jackson suggested that intracranial tumor caused optic neuritis by its irritating effect, acting as a foreign body in the skull. This theory has been supported by Brown-Séquard, and in 1868 by Benedikt; the latter ascribed the effect to irritation of the vaso-motor nerves. This theory assumes that the tumor acts as a source of irritation, producing a reflex influence through the vaso-motor nerves upon the optic disc, and thus leading to its inflammation. This theory, however, has been rejected by most writers on the ground that it involves a mechanism which is not known positively to exist. It is still regarded by Hughlings Jackson, however, as the theory which best explains the phenomena of neuritis.

Edmunds and Lawford hold that optic neuritis, when due to an intracranial cause, is secondary to basilar men-

ingitis, and that the inflammation reaches the substance of the nerve trunk through its sheath.

Parinaud believes that neuritis is invariably the effect of distention of the ventricles of the brain, which causes general cerebral œdema.

In our endeavors to elucidate the truth of the causation of "choked disc," or papillitis, there are certain facts to be remembered. Optic neuritis, limited to or most intense in the optic disc, may occur without any apparent intracranial disease. Pure papillitis is known to occur in simple anæmia. From a consideration of these facts it seems fair to conclude that the intra-ocular end of the optic nerve is a structure peculiarly prone to inflammation.

It is a difficult matter to connect papillitis with increase of intracranial pressure, for it is the rare exception in chronic hydrocephalus, where the intracranial pressure is raised to the highest point of which we have any knowledge. On the other hand, in cases of intracranial tumor with papillitis, there may be no sign of increased intracranial pressure during the life of the patient. There may also be signs of increased intracranial pressure in cases of tumor without papillitis. If we reject the theory that pressure on the cavernous sinus is the immediate cause of papillitis, still we cannot absolutely ignore its influence on the retinal circulation. The great distention of the veins and the narrowing of the arteries occur mainly when the inflammatory process has reached a high degree of intensity, and these facts point to the inflammation in the nerve as the cause of the strangulation by pressure on the vessels, and this view is confirmed by pathological investigations. The conspicuous constriction of the vessels is always in the papilla, in front of the sclerotic. Moreover, the most intense signs of strangulation may be seen in cases in which there is no reason to suspect the presence of intracranial disease. Distention of the optic sheath is frequently met with in cases of papillitis, but it is by no means invariable either in cases of tumor or in

conditions of increased intracranial pressure. It may be absent in cases of cerebral tumor with papillitis. It may also be absent in cases of tumor with internal hydrocephalus.

It has been suggested that the fluid may be found within the sheath itself. If the sheath is the main lymph channel by which fluid is conveyed from the eye, its distention in optic neuritis by fluid escaping from the papilla is easily understood. But there is good ground for believing that the fluid found in the sheath passes into it from the subarachnoid space. We really know very little of the relation which may exist between dropsy of the sheath and optic neuritis. The occasional occurrence of papillitis without it shows that it is neither the invariable nor the chief mechanical cause of papillitis.

It therefore seems proper to draw the conclusion that we cannot decide in any given case against the existence of a descending neuritis from examination of a small portion of the trunk of the optic nerve, and that a pathological change in the nerve, deviating but slightly from the normal state, may convey a condition of irritation to the eye which is sufficient to set up actual papillitis.

It also seems proper to draw the following conclusions in regard to the development of papillitis in intracranial disease:

1. In cases of cerebral tumor, evidence of descending inflammation may be traced in the sheath or nerve much more commonly than is generally supposed, while in cases of meningitis the evidence of descending inflammation is almost invariable.

2. The resulting papillitis may be slight or may grow intense, but we are ignorant of the causes which bring about this difference.

3. The mechanical congestion in these cases of papillitis does not always result from compression of the vessels behind the sclerotic ring, but does always follow compression by inflammatory exudation in the papilla.

4. Slow increase of intracranial pressure has no effect

on the retinal vessels, but a sudden increase of such pressure may intensify a papillitis originating in some other way.

5. Distention of the sheath alone is probably not sufficient to cause papillitis by its mechanical effect, but may intensify the process otherwise set up.

In seeking to make a diagnosis as to the cause of papillitis in a given case, we must depend largely on the presence or absence of indications of disease of the brain, or of such disease of the general organism as is known to be accompanied by optic neuritis. A high degree of papillitis with intense strangulation is seldom met with except in cases of cerebral tumor and some forms of idiopathic papillitis. The slighter degrees of papillitis, not uncommon in cerebral tumor, chronic meningitis, and other intracranial diseases, and the neuritis which occurs in Bright's disease, resemble each other closely. It is upon the presence of other symptoms that the diagnosis in disputed cases must rest. In considering cerebral disease as a cause, it must not be forgotten that papillitis due to a cerebral tumor may be unaccompanied for some time by any signs of intracranial disease, while on the other hand a papillitis due to some general organic disease may be accompanied by some symptoms suggestive of cerebral disease.

In *cerebral hyperæmia* there is no sufficient evidence to show that the vascularity of the disc participates in any transient cause of cerebral congestion, unless the whole head suffers. In most of the cases of long-continued vascular disturbance there are also signs of grave functional disturbance of the brain. The absence of any marked vascular alteration in the eye in cases of disturbed cerebral circulation is abundantly supported by the testimony of skilled observers.

Inflammation of the Brain.—We know nothing of any ophthalmoscopic changes in acute inflammation of the brain without meningitis. In so-called "chronic encephalitis" there may be very marked papillitis, similar to that

found in cerebral tumor, due probably to the propagation of some irritative process from the brain along the nerves.

Gowers thinks that in the rare cases in which hæmorrhage or softening from embolism or thrombosis causes papillitis, the effect is probably produced through the agency of a secondary inflammation.

Hæmorrhages on the *disc* or in the *retina* are met with in a considerable number of cases of cerebral hæmorrhage. Their most frequent cause is the granular contracted kidney. They indicate the existence of conditions which favor degeneration and rupture of the vascular walls, and they are often associated with cardiac hypertrophy.

Disease of the kidneys and *gout* may explain the occurrence of papillitis when it exists alone, apart from other symptoms. Isolated double papillitis may be due to syphilis.

In *abscess of the brain*, papillitis resembling that due to cerebral tumor is often met with, but it is also frequently absent.

Tumors of the Brain.—Optic papillitis is *the* ocular lesion in intracranial growths. It is present in various degrees in a large proportion of the cases of tumor. It is not always possible to say on what the occurrence of papillitis depends. The position of the growth has apparently no direct influence on its occurrence, and the indirect influence depends upon the fact that the secondary meningitis at the base is more extensive when the tumor is not far from that part of the base. The nature of the growth does not influence the development of papillitis, for it may occur with all forms of intracranial growth. Slowly growing tumors seem to have less tendency to cause papillitis than those which grow rapidly. Hence we may conclude that intracranial tumors do not cause papillitis by the direct effect of their mass on the intracranial pressure. In most cases papillitis is a transient event in the history of a cerebral tumor, and is not a constantly associated symptom. A tumor may exist and cause

symptoms of cerebral disturbance for some time, without leading to any changes in the eye. Papillitis may then be developed, run its course, and end in atrophy of the nerves, while the symptoms of the tumor continue for months or even years.

The appearance of the discs in papillitis due to tumor is that of papillitis in its most typical form. It is in most cases double, but often more intense in one eye than in the other. It is probably true that the occurrence of papillitis indicates progress in the growth of the intracranial tumor. If the progress of the latter is arrested or diminished, the papillitis usually subsides and may disappear completely.

The value of papillitis as an indication of the existence of an intracranial tumor is certainly very great. It may be the only unequivocal sign of the presence of organic cerebral disease. It may also give us signs on which to base a prognosis. A subsidence of papillitis may be regarded as indicating in most cases a retrogression of the growth, and a papillitis of very chronic course may indicate that the progress of the tumor is equally slow.

Papillitis of the usual type is frequently present in *hydatid cysts* of the brain.

The papillitis met with in *intracranial aneurism* is probably a descending inflammation, extending to the nerve from the inflammatory process which always exists around an aneurism.

In *simple acute meningitis* of the base, papillitis may occur by direct propagation along the nerve. It is very rare in meningitis of the convexity.

In *tubercular meningitis* papillitis is very common.

Von Graefe regarded this form of nerve inflammation as a typical example of descending neuritis, the inflammation passing directly from the membranes to the optic nerves. It may, however, be a true papillitis, and is always double. The same may be said of *syphilitic meningitis* of the base, and of *traumatic meningitis*, though here

papillitis is rare. *Meningeal growths* very frequently cause papillitis.

In diseases of the *cranial bones* papillitis may occur, but only as the result of meningitis and abscess. The same may be said of *injuries* to the *head*, and of *contusion* and *laceration* of the *brain.*

Diseases of the Nose and Sinuses of the Cranial and Facial Bones.—Some interesting cases have been reported in which papillitis coincided with a persistent discharge of a watery fluid from one or both nostrils. In most of these cases there were also symptoms of chronic cerebral disturbance. The most probable explanation of these cases is that there was an increased intracranial pressure as from internal hydrocephalus, and that the escape of the fluid relieved the pressure indirectly.

Diseases of the Spinal Cord.—Myelitis may be accompanied by papillitis. In *injuries to the spine* the changes in the disc are those of simple congestion with œdema.

Bright's Disease.—In the neuritic form of the intra-ocular complications of kidney disease, papillitis predominates largely over the retinal changes. The arteries are usually narrow and often concealed, and there may be small white dots on the surface of the disc, with hæmorrhages.

In *diabetes* changes in the fundus are rare, but there may be true papillitis, as well as retinitis, with hæmorrhages. In this connection it is well to remember that papillitis and glycosuria may both be consequences of an organic cerebral disease.

Acute Anæmia from Hæmorrhage.—If the eyes of such patients are examined early in their course, signs of neuro-retinitis are commonly observed, sometimes very intense. Von Graefe held that in these cases there is a retrobulbar hæmorrhage. Ulrich considers that the papillitis in these cases is due to a disturbance in the circulation of the papilla, and he attributes the later changes to the establishment of an abnormal relation between the blood pressure and the vitreous pressure.

In *leucocythæmia* papillitis has been met with, and in

some of these eyes which have been examined microscopically, the papilla was found swollen from œdema and infiltrated with leucocytes.

Sudden suppression of the menses has not infrequently been known to cause acute papillitis, and the same has also been seen in chronic menstrual irregularities.

Papillitis, with and without hæmorrhages, has also been occasionally met with in the eruptive fevers and in malarial fevers, independently of any renal complications.

SCIENTIFIC WORK

IN THE

SECTIONS OF THE ACADEMY.

SECTION ON GENITO-URINARY DISEASES.

DECEMBER 9TH, 1890.—F. R. Sturgis, M.D. Paper: "The diagnostic value of hematuria in affections of the genito-urinary organs."

R. W. Taylor, M.D. Paper: "Chancres of the fingers: their clinical history; their complicating septic infection; and their danger in surgical and obstetrical practice." Published, Med. Record, 1891, xxxix., 69.

SECTION ON LARYNGOLOGY AND RHINOLOGY.

JANUARY 28TH, 1890.—J. S. Nichols, M.D. Paper: "A method of correcting adhesions between the soft palate and the pharyngeal wall." Published, N. Y. Med. Jour., 1890, li., 219.

J. O'Dwyer, M.D. Paper: "Report of a case of retained tracheal canula treated by intubation of the larynx."

FEBRUARY 25TH, 1890.—J. M. W. Kitchen, M.D. Paper: "On the causation of diseased conditions in the larger respiratory passages."

J. N. Mackenzie, M.D., of Baltimore. Paper: "What is the correct interpretation of the relationship between nasal disease and asthma?"

MARCH 25TH, 1890.—W. F. Chappell, M.D. Paper: "Sarcoma of the tonsil."

R. C. Myles, M.D. Paper: "Practical anatomy of the nose associated with the pharynx and ear."

MARCH 25TH, 1890.—W. C. Jarvis, M.D. Paper: "The etiology and radical treatment of nasal myxomata."

APRIL 22D, 1890.—W. Freudenthal, M.D. Paper: "Translumination of the larynx and of the antrum of Highmore." Published, Med. Record, 1890, xxxvii., 558.

W. F. Chappell, M.D. Paper: "Neurasthenia and neuralgia from traumatism of the nasal passages." Published, Med. Record, 1890, xxxvii., 522.

MAY 27TH, 1890.—F. J. Quinlan, M.D. Paper: "Report of a case of electro-cauterization of the middle turbinated bone followed by meningitis." Jour. Respir. Organs, N. Y., 1890, ii., 100.

O. B. Douglas, M.D. Paper: "The work of the throat department of the Manhattan Eye and Ear Hospital."

H. Schweig, M.D. Paper: "The use and abuse of the galvano-cautery in throat practice." Published, N. Y. Med. Jour., 1890, lii., 177.

OCTOBER 28TH, 1890.—D. B. Delavan, M.D. Paper: "Translumination of the larynx. New method."

C. H. Knight, M.D. Paper: "A sequestrum removed from the nasal fossa by Ronge's method." Published, Med. News, Phila., 1891, lviii., 6.

D. B. Delavan, M.D. Paper: "The surgical treatment of tubercular laryngitis." Published) Med. Record, 1890, xxxviii., 540.

NOVEMBER 25TH, 1890.—F. H. Bosworth, M.D. Paper: "Anatomy and physiology of the tonsils."

C. C. Rice, M.D. Paper: "Etiology and treatment of suppurative tonsillitis." Published, Med. Record, N. Y., 1891, xxxix., 125.

DECEMBER 23D, 1890.—J. W. Gleitsman, M.D. Paper: "Experience with trichloracetic acid in two hundred cases of affections of throat and nose."

SECTION ON OPHTHALMOLOGY AND OTOLOGY.

JANUARY 20TH, 1890.—H. Knapp, M.D. Paper: "Tuberculosis of the conjunctiva." Published, Arch. Ophthal., 1890, xix., 78.

FEBRUARY 17TH, 1890.—C. J. Kipp, M.D., of Newark. Paper: "Gonorrhœal irido-choroiditis." Published, Med. Rec., N. Y., 1890, xxxviii., 39.

MARCH 17TH, 1890.—R. Kalish, M.D. Paper: "Arrest and partial resorption of immature cataract, with restoration of reading power." Published, Med. Rec., 1890, xxxvii., 341.

P. A. Callan, M.D. Paper: "A case of double exophthalmus."

APRIL 21ST, 1890.—C. J. Blake, M.D., of Boston. Paper: "Mechanical experiments in the treatment of middle ear diseases."

MAY 21ST, 1890.—C. S. Bull, M.D. Paper: "The surgical treatment of fixed membranous opacities in the vitreus." Published, Ophthal. Rev., Lond., 1890, ix., 161.

D. Webster, M.D. Paper: "A case of paralysis of the superior oblique caused by tenotomy of the superior rectus of the same eye." Published, Med. Rec., 1890, xxxvii., 654.

OCTOBER 20TH, 1890.—M. L. Foster, M.D. Paper: "Cyst of the lachrymal gland." Published, Arch. Ophthal., N. Y., 1891, xx., 53.

NOVEMBER 17TH, 1890.—H. Richards, M.D. Paper: "Implication of the mastoid cells of both sides complicating otitis media purulenta. Complete recovery after operation." Published, N. Y. Med. Jour., 1891, liii., 68.

A. Rupp, M.D. Paper: "Syphilis in ear diseases."

NOVEMBER 17TH, 1890.—G. B. Hickok, M.D. Paper: "Treatment of acute and chronic middle ear diseases."

SECTION ON ORTHOPÆDIC SURGERY.

JANUARY 17TH, 1890.—V. P. Gibney, M.D. Paper: "The operative treatment of talipes calcaneus paralyticus." Published, Boston Med. and Surg. Jour., 1890, cxxii., 205.

MARCH 21ST, 1890.—A. B. Judson, M.D. Paper: "A criticism of Willet's operation for talipes calcaneus." Published, N. Y. Med. Jour., 1890, lii., 198.

R. Whitman, M.D. Paper: "The rational treatment of flat foot." Published, N. Y. Med. Jour., 1890, lii., 406.

OCTOBER 17TH, 1890.—T. H. Myers, M.D. Paper: "Treatment of ankle joint disease." Published, Bost. Med. and Surg. Jour., 1890.

DECEMBER 19TH, 1890.—J. Ridlon, M.D. Paper: "Non-operative treatment of delayed union in fractures of the leg." Published, Med. Record, 1891, xxxix., 130.

W. R. Townsend, M.D. Paper: "Uniform nomenclature in orthopædic surgery."

The meetings of February, April, and November were given up to the reports of clinical cases and the exhibition of apparatus.

SECTION ON NEUROLOGY.

JANUARY 10TH, 1890.—C. L. Dana, M.D. Presentation of a case of Friedreich's ataxia, and report of six other cases occurring in three families. Published, Jour. Nervous and Mental Dis., March, 1890.

APRIL 11TH, 1890.—Mary Putnam Jacobi, M.D. Paper: "Hysterical fever." Published, Jour. Nervous and Mental Dis., June, 1890.

OCTOBER 10TH, 1890.—This month a general meeting of the Academy was held under the auspices of the section.

SECTION ON OBSTETRICS AND GYNÆCOLOGY.

JANUARY 23D, 1890.—J. R. Goffe, M.D. Paper: "A new method, the intra-abdominal but extra-peritoneal method, of disposing of the pedicle in supravaginal hysterectomy for fibroids, with report of four successful cases." Published, Amer. Jour. Obstet., 1890, xxiii., 372.

FEBRUARY 27TH, 1890.—A. H. Goelet, M.D. Paper: "The treatment of pelvic hæmatocele by galvanism." Published, Gaillard's Med. Jour., 1890, l., 342.

H. C. Coe, M.D. Paper: "How soon after delivery does the responsibility of the accoucheur cease?" Published, Gaillard's Med. Jour., 1890, l., 451.

MARCH 27TH, 1890.—A. F. Currier, M.D. Paper: "A case of uterus bicornis unicollis with parenchymatous abscesses of the posterior vaginal wall." Published, Ann. Gynæcol. and Pædiat., 1889-90, iii., 321.

R. A. Murray, M.D. Paper: "The Cæsarean section in moderately contracted pelvis." Published, N. Y. Med. Jour., 1890, li., 673.

APRIL 24TH, 1890.—C. D. Scudder, M.D. Report of a case of kyphotic pelvis.

H. J. Boldt, M.D. Paper: "Salpingo-oöphorectomy and its results." Published, Med. Record, 1890, xxxvii., 545.

MAY 22D, 1890.—I. H. Hance, M.D. Paper: "The immediate repair of injuries to the pelvic floor." Published, Med. Rec., N. Y., 1890, xxxviii., 228.

H. C. Coe, M.D. Paper: "Artificial prolapse of the uterus; its risks." Published, Med. Record, N. Y., 1890, xxxviii., 141.

OCTOBER 23D, 1890.—R. Waldo, M.D. Paper: "Pregnancy complicated by peri-uterine inflammatory deposits."

G. M. Edebohls, M.D. Paper: "Exploratory puncture of the female pelvic organs." Published, Med. Record, 1890, xxxviii., 568.

NOVEMBER 28TH, 1890.—J. C. Edgar, M.D. Paper: "The manikin in the teaching of practical obstetrics." Published, N. Y. Med. Jour., 1890, lii., 701.

F. Krug, M.D. Paper: "Transperitoneal hysterorrhaphy: a new method of ventro-fixation of the uterus without opening the abdominal cavity." Published, N.Y. Med. Jour., 1891, liii., 11.

SECTION ON PÆDIATRICS.

JANUARY 9TH, 1890.—F. Huber, M.D., presented a patient who had had empyema complicated by croupous pneumonia and abscess.

Robert Abbe, M.D. Paper: "Treatment of empyema."

FEBRUARY 13TH, 1890.—E. L. Partridge, M.D., reported a case of sudden death of an infant from an unusual cause.

A. Seibert, M.D., presented apparatus and gave rules for infant feeding based on weight, not on age.

J. L. Smith, M.D. Paper: "Investigations showing the increase of weight after feeding."

MARCH 13TH, 1890.—S. E. Milliken, M.D., presented a patient—femoral hernia in a boy of sixteen months.

T. M. Crandall, M.D., reported two cases of gonorrhœa in a brother and sister aged six and eight years respectively.

H. Koplik, M.D. Paper: "Arthritis complicating vulvo-vaginitis in children." Published, N. Y. Med. Jour., 1890, li., 678.

MARCH 13TH, 1890.—C. G. Kerley, M.D., reported twenty cases of tuberculosis in infants.

Discussion on the "Prophylaxis of diphtheria."

APRIL 10TH, 1890.—J. L. Smith, M.D., presented a case of hydrocephalus following cerebro-spinal fever in a boy of ten months.

F. Huber, M.D., presented a case of empyema of left side with œdema of right lung in a girl of twenty months.

T. M. Crandall, M.D., showed a calculus which had been impacted in the urethra of a boy of three years.

I. H. Hance, M.D., showed a calculus from the kidney of a girl of twenty months.

W. L. Carr, M.D., presented a photograph of malformations of fingers in a girl of two years.

L. E. Holt, M.D. Paper: "Histories of cases of pneumonia with low temperature." Published, N. Y. Med. Jour., 1890, lii., 67.

A. Jacobi, M.D. Paper: "Effects of spirits and malted drinks on nursing women."

MAY 8TH, 1890.—W. L. Stowell, M.D., presented a patient—a boy—who had had cerebro-spinal meningitis followed by blindness, from which he had recovered.

J. H. Emerson, M.D., reported a case of congenital stenosis of the duodenum, with specimen.

D. Brown, M.D., presented a specimen of imperforate rectum, and gave the history of a case of erysipelas in an infant. Published, Archives of Pædiatrics, July, 1890.

G. W. Rachel, M.D., gave the history of a case of diphtheritic paralysis involving the extremities and most of the respiratory muscles.

Discussion: Use of antipyretic drugs in pneumonia of infancy and childhood.

OCTOBER 8TH, 1890.—H. D. Chapin, M.D., presented an infant of thirteen months with imperforate rectum.

L. E. Holt, M.D., presented a specimen from a case of hydrocephalus in a child of three weeks.

W. L. Stowell, M.D. Paper: "Study of one hundred cases of pneumonia in children." Published, Med. Record, 1890, xxxviii., 485.

NOVEMBER 13TH, 1890.—W. Mendelson, M.D. Paper: "How to obtain the best results with the milk sterilizer."

H. Koplik, M.D. Paper: "Administration of sterilized milk in dispensary practice." Published, N. Y. Med. Journ., 1891, liii., 123.

J. L. Smith, M.D. Paper: "Peritonitis in infancy and childhood."

DECEMBER 11TH, 1890.—H. D. Chapin, M.D., reported a case of spina bifida.

W. P. Watson, M.D., showed a specimen of anencephalous monstrosity.

L. E. Holt, M.D., presented a girl of four years, showing recovery from symptoms indicating tumor of the brain.

W. E. Forest, M.D. Paper: "Observations upon the influence of artificial respiration on the heart of the newly-born."

B. Scharlau, M.D. Paper: "Treatment of large serous effusions in the chest by free incisions."

SECTION ON PUBLIC HEALTH, HYGIENE, ETC.

JANUARY 3D, 1890.—Henry E. Crampton, M.D. Paper: "The necessity for the enforcement of the law entitled 'An Act to provide for the establishment of municipal lodging houses in New York City' (passed June 2d, 1886, Chap. 535 of Laws of New York)." Published in the Medical Record, 1890, xxxvii., 62.

NOVEMBER 7TH, 1890.—C. G. Currier, M.D. Paper: "Self-purification of water, specially in flowing streams." Published, Am. Jour. M. Sc., 1890, c., 586.

DECEMBER 5TH, 1890.—Simon Baruch, M.D. Paper: "A study of public baths, together with an inexpensive method for their hygienic utilization."

SECTION ON SURGERY.

JANUARY 13TH, 1890.—J. E. Kelly, M.D. Paper: "On the operative treatment of hernia."

E. Fuller, M.D. Paper: "On a rare case of urinary extravasation down the thigh." Published, N. Y. Med. Jour., 1890, li., 159.

FEBRUARY 10TH, 1890.—W. T. Bull, M.D. Paper: "On the results of treatment of fracture of the patella without operation." Published, Med. Record, 1890, xxxvii., 313.

MARCH 10TH, 1890.—C. Koller, M.D. Paper: "Immediate blindness of one eye a symptom of fracture through the base of the skull." Published, N. Y. Med. Jour., 1890, li., 406.

C. A. Powers, M.D. Paper: "On conical stump after operation in children, with special reference to its physiological causes and prognosis." Published, Med. Record, 1890, xxxvii., 641.

APRIL 14TH, 1890.—Exhibition of clinical cases.

OCTOBER 13TH, 1890.—J. B. Roberts, M.D., of Philadelphia. Paper: "Lessons taught by three fatal abdominal operations." Published, Univ. M. Mag., Phila., 1890, 1, iii., 153.

NOVEMBER 10TH, 1890.—J. Woodbury, M.D. Paper: "On vertebral fracture." Published, N. Y. Med. Jour., 1890, lii., 667.

DECEMBER 8TH, 1890.—W. T. Bull, M.D. Paper: "Three cases of pylorectomy with gastro-enterostomy

for cancer of the stomach." Published, Med. Record, N. Y., 1891, xxxix., 39.

SECTION ON THEORY AND PRACTICE OF MEDICINE.

JANUARY 21ST, 1890.—W. Mendelson, M.D. Paper: "The physiological treatment of obesity." Published, Med. Record, 1890, xxxvii., 176.

Louise F. Bryson, M.D. Paper: "Epidemic influenza." Published, N. Y. Med. Jour., 1890, li., 120.

FEBRUARY 18TH, 1890.—B. F. Westbrook, M.D. Paper: "The mortality rate from pulmonary phthisis in New York and Brooklyn."

S. Baruch, M.D. Paper: "The treatment of typhoid fever." Published, Med. Rec., N. Y., 1890, xxxvii., 318.

MARCH 18TH, 1890.—S. S. Burt, M.D. Paper: "Pulmonary tuberculosis viewed in the light of modern research." Published, Med. Record, 1890, xxxvii., 397.

J. H. Linsley, M.D.: Exhibition of photomicrographs of tubercle bacilli and tissues.

APRIL, 1890.—General Academy meeting, in charge of the section.

MAY 20TH, 1890.—W. B. Wood, M.D. Paper: "The after-treatment of obstetric cases." Published, N. Y. Med. Jour., 1890, lii., 126.

J. W. Roosevelt, M.D. Paper: "Gross anatomy of chronic pulmonary consumption in relation to diagnosis and prognosis." Published, N. Y. Med. Jour., 1890, lii., 58.

M. Einhorn, M.D. Description and demonstration of an instrument for obtaining gastric fluids for examinations. Published, Med. Record, N. Y., 1890, xxxviii., 63.

OCTOBER 21ST, 1890.—G. R. Lockwood, M.D. Paper: "Acute hæmorrhagic purpura and allied diseases." Published, Med. Record, 1891, xxxix., 155.

NOVEMBER 18TH, 1890.—J. C. Minor, M.D. Paper: "Indications for trephining in epilepsy." Published, Med. Record, N. Y., 1891, xxxix., 31.

DECEMBER 16TH, 1890.—J. W. Roosevelt, M.D. Paper: "Anatomy of the lungs illustrated by corrosion specimens."

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